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PREFACE.

THE following Lectures were given at the conclusion of the course of Chemistry at St. George's Hospital, in 1849. I had intended to illustrate them by cases, and to form them into a treatise on Stomach and Renal Diseases; but circumstances have determined me to re-print them, with some corrections, from the original notes.

How far I am indebted to the great work of Dr. Prout, those only can know who have been experimentally occupied with animal chemistry. He first established the true connexion between chemistry and medical practice; and though the chemist sees that discoveries are being made in each portion of his science with increasing rapidity, yet the physician will daily be more and more convinced of the value of those practical rules which he owes to the wisdom and to the accuracy of Dr. Prout.

June 1st, 1850.

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ERRATA.

Page 47, line 7, *for* 9½ P.M., *read* 2½ P.M.
47, line 8, *for* 9½ *read* 6½.

LECTURES.

LECTURE I.

ON FOOD.

Gentlemen,

I have undertaken to give you twelve lectures on animal chemistry, a subject on which twelve times twelve lectures would hardly be sufficient to make you acquainted with the present state of our knowledge.

A superficial view of each portion of animal chemistry would give you very little indeed that was either clear or practical; and, on this account, I intend to take only one small portion—viz. the chemistry of the urine, which admits of no splendid experiments, or attractive illustrations, but which, if it can be made clear to you, may be of daily use to you in the diagnosis and treatment of stomach and renal diseases.

My first three lectures will be on food, respiration, digestion, and the blood; chiefly to enable you to see their influence on the urine. The quantitative and qualitative variations in this excretion will be the subject of the other nine lectures.

Such will be my present course; at some future period I may take some other portion of animal chemistry, as the

blood, or digestion, and make it the subject of my lectures; but neither these, nor any other subjects, approach the one I have chosen in interest for medical men. Dr. Bright's discovery has made a knowledge of the urine essential to every one of us; and the microscope, with chemical reactions, enables us to see diseases of the urinary organs, and to detect disorders of the system dependent on them, or on indigestion, with as much clearness as the stethoscope enables us to see diseases of the heart and of the lungs.

All of you, I am sure, have already sought, or will before long seek, for clearness in your diagnosis of disease, and for certainty in your prognosis and treatment. You will then know how much we owe to Laennec and to Bright, and, if a third name might be added to these, for advancing medical knowledge, you will all of you agree with me, I doubt not, in acknowledging our debt to Dr. Prout.

I intend to occupy your attention to-day with the subject of food.

Until the last few years, vegetable and animal food were considered to be chemically different. Nitrogen was thought to characterize animal food, while the absence of nitrogen was supposed to distinguish vegetables; and though nitrogen was known to exist in gluten, and in the cruciferæ, it was considered to be peculiar to these; and the production of ammonia in the gas works was passed over as "curious." It is only lately, that highly complex nitrogenous substances have been proved to be universally present in all vegetables, and vegetable albumen is the name given to the most universally diffused nitrogenous compound. I hope, before the close of this lecture, to make the importance of this discovery evident to you.

We take into our bodies gases, liquids, and solids, all the three forms in which matter can occur. We are dependent directly, or indirectly, on air, water, and vegetables, for our

nourishment. That all flesh is grass was long since stated as truth, and we will, for a few moments, examine this grass, and see what it is composed of.

We can dry it, and make hay. We can burn it, and get the ashes. We can treat it with ether, and we dissolve out a green colouring matter with fat or wax, and, by alcohol, we can separate these. Woody fibre is another constituent; and, lastly, we can obtain from the grass an albuminous compound, in small quantity, containing carbon, hydrogen, oxygen, nitrogen, sulphur, and phosphorus, which is vegetable albumen.

No two vegetables consist precisely of the same constituents; but in all, of every kind, we find these four great divisions: 1st, water; 2nd, salts or ashes; 3rd, organic substances, containing very little or no nitrogen, consisting chiefly of carbon, hydrogen, and oxygen, as fatty substances, vegetable acids, starch, sugar, woody-fibre and colouring matter; 4th, organic substances containing much nitrogen, as, for example, vegetable albumen, and albuminoid substances.

Take any vegetable whatever, and you can find these four classes of compounds in it, and, besides these, air alone is necessary for the support of animals. No division can be made into essentials and incidentals, each and all are necessary to keep up the full nourishment of every part of an animal, to supply food for the nerves, the muscles, the cellular tissue, and the bones. Thus we nearly come back to the omoiomeric view of Anaxagoras, when we say that food contains some bony, some fleshy particles, some fatty particles, and some watery ones. (Lucretius, i. 865.)

Some idea of the comparative value of these different classes of substances is obtained, by noting the rapidity with which animals die when any one of them is absent from their food. Thus, the following order might be formed, 1st, air; 2nd, water; 3rd, organic substances containing nitro-

gen; 4th, organic substances free from nitrogen; 5th, inorganic substances, the ashes, salts. The four last classes, which exist in all vegetables, pass in through the stomach, but there is no fixed or constant proportion of these classes of substances in all kinds of plants. The proportions vary very greatly in different species; indeed, in no two specimens of the same species are the proportions always precisely and invariably the same.

If we contrast the juice of the sugar-cane with potatoes, or with rice and wheat, you will see that there is a considerable difference in the quantities of these different classes, but that they are all present. Hence, for the most part, one vegetable differs from another more in the quantities of the different substances present in it than in the quality of its constituents.

	Sugar-cane Juice.	Potatoes.	Rice.	Wheat.	Cow's Milk.	Veal.	Carp.
Nitrogenous substances: Gluten, albumen	0.12	2.49	6.27	13.96	4·5	20.00	19.00
Non-nitrogenous substances: Starch, sugar, fat	Sugar, 20.94	Starch, 17.98	73.65	47.57	Sugar, 4.0		
		Fat, &c. 3.60	4.57	20.74	Butter, 5.5	Fat, 2.29	1.11
Ash	0.14	0.90	.30	2.36	0.5	.08	0.11
Water, when fresh	78.80	74.95	15 14	15.43	85 5	77.63	79.78
	100.00	99.92	99.93	100.06	100 00	100 00	100.00

The potato contains a small amount of albuminous substance, a large quantity of starch or non-nitrogenous substance, some fatty matter, and a small amount of ashes; that is, much non-nitrogenous matter and but little nitrogenized substance, little ash, and much water. Yet, on potatoes, with air and water,—on these alone,—human beings may live. All the substances requisite for the support of the body are present in potatoes, water, and air. Poor living, truly; but why poor? The answer is, not that

there is but little nitrogen present in these substances, for there is an unlimited supply of nitrogen in the air, but the amount of albuminous substance—that is, of organic substance containing nitrogen—is very small in potatoes and air. It is, then, the amount of nitrogen, as indicating the quantity of albuminous substance present in any vegetable, and not the amount of carbon, which determines the value of the nourishment it contains. The seeds of vegetables for the most part contain the greatest proportion of albuminous substance; and hence wheat and other seeds form the most nourishing vegetable food. Why, then, is this vegetable albumen so nourishing, so necessary? The reason is, because it is almost, if not quite, identical in chemical composition and properties with the albumen or fibrin of animals. The muscles of the body have nearly the same composition as the vegetable albumen. Hence the nitrogenous compound of wheat can most easily supply the waste of the muscles from labour; and as wheat contains much more albuminous substance than potatoes, much less of the former than of the latter will be required to repair a given quantity of muscular structure.

It is necessary that you should clearly distinguish food that can form flesh, from food that can form fat. On olive oil, linseed oil, or cod-liver oil, an animal may grow fat, but he cannot work on such food alone. For the work of the muscles, it is requisite that the food should be albuminous, not fatty. A horse, for example, for hard work, must have corn, and not hay. To do work, food rich in nitrogen is necessary. To grow fat, food rich in carbon is required; and this also is because animal fat is, like vegetable fat, rich in carbon, hardly to be distinguished the one from the other. The excess of vegetable fat taken in, is rapidly deposited in the cellular tissue of the animal. The albuminous substance of the vegetable will not make an animal fat, and vegetable

oil and fat cannot make muscle. We must not assume that animals have this power of chemical transubstantiation. It seems to be one great object of vegetables to prepare the food for animals. Vegetables from the air, the water, and the soil, make compounds which animals require—substances, that is, which, with the least possible change, can be formed into parts of the animal. Vegetables from carbonic acid, ammonia, water, and salts, form albuminous substances, fatty matters, starch, sugar,—whereby the various organs of the body are nourished and enabled to perform their actions. The power of forming higher compounds out of more simple ones is, in vegetables, unlimited. Their power of thus compounding simple substances into more complex ones is most extensive; whilst, in animals, the power of conversion is probably limited to a simplifying change—the power of forming higher organic compounds out of lower ones is at present unproved.

But, you might say, cannot animals make nervous substance, the noblest substance in creation, whose action so utterly forbids our comprehension? If we examine this nervous substance, we find it also consists of the four classes—namely, albuminous substance, fatty substance, ash, and water; and the highly organized muscle contains the same substances in different proportions. The power of animals is shown in their forming the substances which they obtain from vegetables into complex structures and organs, whilst the power of vegetables is shown in the production of new compounds, new arrangements of the elements into higher and more complex bodies, as vegetable acids, alkaloids, neutral substances, albuminous and fatty matters.

The power of adapting forms and making organs, the formative and organizing power is more seen in animals than in vegetables.

The power of building up new compounds, new substances;

the compounding, or substance-making power, is, with some exceptions, perhaps, limited to vegetables.

I have said that the value of food may be determined by the quantity of albuminous substance present in it. If the gluten of wheat was dried, freed from all fat by ether, and from all phosphate of lime by being treated with dilute acid, it would not suffice for food. The experiment has failed with white of egg, a substance almost identical in composition with the albumen of vegetables, and the animals fed on it alone died of starvation. The same results follow from a diet of sugar or fat alone; and if nitrogenous and non-nitrogenous food was given perfectly free from all saline matter, perfect nourishment of the body could not take place. Still more than this, if one single salt which exists in the body was left entirely out of the food, disease, if not death, would result. For example, you all have heard how want of phosphate of lime produces rickets. So far, then, as the quality of the substances contained in each vegetable is concerned, every one of them which does not contain an excess of woody fibre, or green colouring matter, might be taken as the type of the food of man. If we take the more universally received type of food, milk, and analyze it, we find precisely the same four classes of substances present in it as in vegetables. The proportions of the ingredients are no doubt those best suited to the wants of the infant, and no doubt every substance present in milk is in that proportion which is best for the nourishment of the child. It is the type of the food for infants, but it is not more the type of the food of man than wheat, for example, is. We have in the milk, 1st, water; 2nd, the casein, or curds, the albuminous, or nitrogenized substance; 3rd, the non-nitrogenous substance, the cream, or oily and fatty matter, and the milk sugar; and 4th, the saline substances, which, when burnt, give ashes.

Lastly, if we take a carnivorous animal, the flesh he feeds on contains the four classes: water; fibrin, or albuminous substance; fat; ashes or salts. Thus, meat or milk, vegetable food or animal food, contain the four kinds of substances which are requisite for man.

Since, then, in all kinds of food we find these four classes of substances present; since we find that animals contain not only the same elements, but almost the same proximate principles and salts, as vegetables; since we have positive experiments, that no one of the four classes of substances alone is able to nourish an animal, we may give up all theories of the conversion of one class of substances into another. I can no more believe that animals can change starch or oil into albumen, than that they can change water into chloride of sodium, or phosphate of lime into urate of ammonia; I have no proof of such chemical transubstantiation.

We owe the present clearness of our views to Professor Liebig, who first brought prominently forward the universal presence of albuminous substances in vegetables, and we owe to him also the highly probable conjecture as to the use of the non-nitrogenous substances which we take as food; the sugar, starch, fat, and oil. If they cannot form muscle they at least can make animals fat. The fat and oil can form an internal clothing, and by being bad conductors of heat they retain the warmth of the body, and are kept ready to be used when the supply of non-nitrogenous matter is deficient in the food. Professor Liebig has named these non-nitrogenous substances elements of respiration; substances taken into the body for the oxygen of the air to act on, whereby carbonic acid, water, and heat are produced, and the temperature of the body is made to rise higher than that of the surrounding medium. If no heat was produced, chemical changes, contraction of the muscles, formation of the mus-

cular and nervous substance, and their various actions, would not so readily take place.

I by no means say that it is impossible for nervous action to produce some of the heat of the body. We know that mechanical action—friction, produces heat; electrical action produces heat; above all, chemical action produces heat, as in the action of oxygen on hydrogen or carbon, and it is not improbable, therefore, that muscular or nervous action should produce some heat. Hence the conclusion at which Sir B. Brodie arrived may be so far true, as that part of the heat of the body may depend on nervous action. But there can be no doubt that the greater part of the heat of the body is due to chemical action, and is the effect of the oxygen we inspire. I must here, at the commencement of these lectures, express my belief, that the clear comprehension and investigation of this action of oxygen in the human body is of no less practical importance than the great and fundamental principle of the circulation of the blood. Before I conclude these lectures, I shall have occasion to bring the action of oxygen in the body frequently to your notice. Here it will be sufficient to point out the relation which should subsist between the supply of oxygen and food. If the non-nitrogenous food or the elements of respiration are chiefly for the production of animal heat by combining with the oxygen inspired, then there must be some relation between the quantity of air inspired and the amount of non-nitrogenized food eaten. If too much oxygen is taken, the body will waste; if too much non-nitrogenized food is taken, the deposit of fat may constitute disease. To preserve the health, there must also be some relation between the amount of muscular action—that is, exercise or labour—and the supply of nitrogenized food. By considering, then, how much air and exercise is taken, we ought to determine how much of the non-nitrogenized and nitrogenized food is required for

the repair of the wasted muscles and tissues, and for the support of the animal heat.

Let me say a few words on this our gaseous food. The air is a mixture of nitrogen and oxygen. It passes into the air-vesicles. In the moisture of the air-vesicles this oxygen gas, and probably some nitrogen gas, must be dissolved. The fluid takes up a portion of these gases, and they are absorbed by the blood. The oxygen, possessing all its chemical properties, circulates to the systemic capillaries, and there chiefly the formation of carbonic acid takes place. The carbonic acid, dissolved in the serum, returns to the lungs, and escapes usually with a slight excess of nitrogen above the quantity inspired.

An easy expiration consists of from fifteen to eighteen cubic inches of air, and this contains about three and a half per cent. of carbonic acid. A deep expiration contains six or eight per cent. of carbonic acid, and will not support combustion. Hence, in twenty-four hours, from seven to fourteen ounces of carbon are thrown out burnt daily, and this carbon is derived from the starch, sugar, fat, &c., taken as food. On these the inspired oxygen acts, and if these are absent from the food, fat is absorbed from the cellular tissue, and burnt.

I do not consider that the oxygen acts on the non-nitrogenous substances alone, for there is evidence of the action of oxygen on the albuminous substances in the body, both on those which have been assimilated, and also on those which have recently been taken in as food.

I shall show you, that if an excess of water, of chlorides, of sulphates, and of phosphates, be taken into the blood, the excess is removed. If an excess of fat or oil is taken, some portion of it also is thrown out. If even an excess of albuminous substance be eaten, a portion of it is acted on by the oxygen inspired, and is thus more easily removed. Ulti-

mately, carbonic acid, ammonia, and water, are formed by the action of oxygen in or out of the body.

Thus, during life and after death, directly or indirectly, the greater part of our bodies escapes in the gaseous form, as carbonic acid gas, ammoniacal gas, watery vapour, sulphuretted and phosphuretted hydrogen. We are ultimately used for the support of vegetable life; from grass we came, and to grass we shall return. Thus, the course of the matter of the body may be represented by a circle; the course of the powers of the mind, on the contrary, by a straight line, but, like the circle, it is infinite, extending into eternity.

LECTURE II.

ON DIGESTION.

In my last lecture, I showed you that all vegetable food contained four classes of substances,—First, Water; Secondly, Ashes; Thirdly, Non-nitrogenous organic substances; Fourthly, Albuminous organic substances. I stated to you that milk, as regards the quality of its constituents, is no more the type of food than wheat is. Both contain the same four kinds of substances—the difference is in the quantity of water present. Without quantitative analysis, this is made clear by the fact that we could live without water being added to our milk, but scarcely without its being taken with our bread.

Bread and water together are the type of the food of man, as milk is of the food of the child. Both contain some fleshy, some fatty, some bony, and some watery particles, but the quantity of water naturally present in milk is sufficient to dissolve the other substances present in it when circumstances favour their solution. What these circumstances are, I purpose making the subject of my present lecture.

The process of digestion has been divided into a process of reduction, a converting process, and a vitalizing process. In my last lecture, I told you that the conversion of a substance belonging to one of the four classes into which food

may be divided, into a substance belonging to another class, is neither proved nor probable, so that your idea of digestion may have this separated from it.

The term reduction arose from sugar which contained much water being sometimes called low sugar. Whether the water was chemically combined with the substance or not, was not very clearly expressed. Instead of the term reduction, I shall always use the word solution, which you will more easily understand. Regarding the so-called vitalizing process, I can form no clear idea of it, and so I shall not, by talking about it, enable you to understand what is meant thereby. You will say, then, does the process of digestion mean only a process of solution and nothing more? I think you will see that digestion must not be so limited. Food is dissolved, then it is absorbed, and then it is said to be made into blood, made like to the blood—assimilated is the one complex word at present used. If the solution and absorption are included in the word digestion, we may speak of food as first digested, and then assimilated to the blood. Rarely the term digestion is used when absorption only is meant, the food being dissolved, or being a liquid before it is taken into the stomach. Thus you may read of the digestion of a mineral water, meaning its absorption. In my lecture to-day, I shall attempt to make clear to you more especially the first of these three different processes—1, solution; 2, absorption; 3, assimilation.

The quantity of water present in vegetables is not sufficient to dissolve the nitrogenous and non-nitrogenous organic substances, and the salts which they contain. Even if these three classes were all soluble in water, there is not enough of this fourth-class, water, present to hold the other classes in solution, and thus enable them to pass into the blood.

The more soluble a substance is, whether nitrogenous or non-nitrogenous, or saline, the more quickly does it pass into

the blood. Some substances,—as sugar, for example,—are readily soluble in water, and water only is requisite to prepare such for absorption. Woody fibre, on the contrary, and green colouring matter, are very insoluble in water, and partly from this chemical property, and partly from mechanical properties as the texture of the woody fibre, they are with difficulty made soluble. Nitrogenous substances, as regards their solubility, differ much in their chemical and mechanical properties. Generally, the more minutely any substance is divided, the more easily will it be dissolved. Division gives surface for the fluids to act on; and the force of cohesion is by the division already overcome.

The first most essential requisite for solution is that some solvent for the food should be present. If the solvent be pure water, then the food must consist of substances soluble in pure water. If the food consists of nitrogenous, non-nitrogenous substances, and salts, then, if these be not soluble in water, the solvent must be some fluid different from pure water. From the best experiments, it is admitted that the solvent in the stomach is an acid liquid. It is shortly named the gastric juice. Before I describe the nature and properties of this solvent gastric juice, I will mention some other points regarding the solution of food. Having obtained a solvent, the second most essential operation is to divide the substance to be dissolved, as finely as possible. That food which is most capable of being finely divided, will for the most part be most easily dissolved. Herein the advantage chiefly consists of tender meat over tough; of stale bread over new; of yolk over the white of a boiled egg; of fish over flesh. The third essential point is, that an excess of the substance to be dissolved should not be taken. A given quantity of water cannot dissolve an unlimited quantity even of sugar or salt. A given quantity of dilute acid cannot dissolve an unlimited quantity of albumen; and

any given quantity of the gastric juice cannot dissolve an unlimited quantity of food. Moreover, if too much non-nitrogenized food is dissolved, the excess must be deposited as fat, or thrown out by the excretions ; and if too much albuminous food is taken, it must accumulate in the blood, or be thrown out of the blood ; and fat and plethora are not the only evils too much food causes. If an excess is taken into the stomach, the irritation causes the greatest possible secretion of gastric juice. If too little gastric juice is poured out, a portion of the food will remain undissolved, and sickness or disorder of the bowels will result. If too much gastric juice is poured out, heartburn, pain in the stomach, and flatulence, are produced. Excessive irritation of the stomach, and its never-failing consequence, excessive acidity, may be produced not only by too much food, but by certain kinds of food. The error may be as great in the quality as in the quantity of food. The bulb of a thermometer, a probang, and metallic balls, have been used to excite the secretion of acid in the stomach ; and hard, solid food or highly stimulating pepper, frequently causes excessive irritation of the stomach, and excessive acidity and flatulence and increased muscular action, sometimes as spasmodic and painful as any cramp in the extremities. These effects depend as much on the quality as on the quantity of food. The mechanical properties of the food, even more than their chemical ones, are concerned in causing this irritation ; but dilute sulphuric acid, vinegar, or sugar even, which can become acid, will cause as much pain as the toughest meat, or the hardest potato.

You see, then, why unirritating food, in the finest state of division, and in moderate quantity, can prevent and cure that form of indigestion which depends on an over irritable state of the mucous membrane of the stomach.

I must return now to the nature of the gastric juice, and

to its action on the different classes of substances of which our food consists.

On the Nature of the Gastric Juice and the Changes it Effects on Nitrogenized and Unnitrogenized Food.—On the 5th of March, 1849, breakfast having been taken at eight A.M., on boiled beef, bread, and spring water, at 9.45, by position and voluntary effort, about a pint of substance was ejected. The first ounces were scarcely acid to the taste; the last portion was most intensely acid. The whole quantity was thrown on a filter, and a clear, yellowish-brown liquid passed through. This clear liquid was intensely acid to test paper. It coagulated slightly on the addition of nitric acid and heat. The cold acid caused a deepish yellow colour and a coagulum, which appeared partly to be soluble by heat and precipitable by cooling. The specific gravity = 1008.2. When 504.1 grains were evaporated in vacuo over sulphuric acid, a small quantity of gas was evolved.

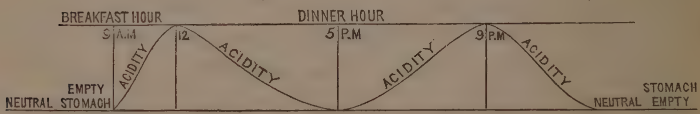
	Residue + basin	= 549.3 grains.
	basin	= 505.0 „
March 15th.	Dry residue	= 44.3 grs. = 8.8 per cent.

The residue was yellowish, semi-transparent, took the impression of the nail, smelt of musk and sugar! When mixed with cold distilled water it became ropy and very adhesive. The solution was highly acid. The taste was sweet, acid, bitter, nauseous. A few drops of the solution, with sulphate of copper and liquor potassæ, gave an intense blue, with a trace of purple; and when heated the reduction of the oxide of copper was very rapid, excessive, and red.

In this bottle you see this gastric juice. Here is its strong acid reaction on litmus, and here you see the rapid reduction of the oxide of copper. The food being bread, it is probable that some of the sugar was formed in the process of fermentation, and not produced altogether in the stomach at the expense of the starch.

This gastric juice, then, is a highly acid fluid secreted by the stomach. It consists—1, of water and free acid; 2, of salts; 3, of non-nitrogenous organic substances; 4, of albuminous or nitrogenized substance. Of these substances the most important is the free acid. What the acid is has not yet been determined. Hydrochloric, phosphoric, acetic, lactic, and butyric acids, have each been said to exist in the gastric juice. The hydrochloric and phosphoric are mineral or inorganic acids. The rest are organic acids,—possibly arising from starch, sugar, or fat, or other components of the non-nitrogenized part of our food. Thus these organic acids might be formed; but whence can the inorganic acids come? Hydrochloric and phosphoric acids exist only in the food and blood, combined with soda, potash, or lime; as in common salt, phosphate of soda, or phosphate of lime. To set free the acids, the alkalies must be separated. If one equivalent of hydrochloric acid is set free in the stomach, one equivalent of soda must be set free in the blood. The greater the quantity of acid in the stomach the greater the quantity of alkali in the blood, and the more alkaline the serum must become. Whether this separation is effected by galvanic action, nervous action, or muscular action, is at present altogether unknown. Those who say anything say it is by vital action. Whatever may be the nature or seat of the decomposing force,—whether galvanic, nervous, or muscular,—whether in the cells of the epithelium of the stomach-tubes, or in the muscular structure,—we cannot admit that inorganic acid can be poured into the stomach without an equivalent quantity of alkali being set free in the blood. When digestion is completed, the acid is re-absorbed with the food, and the alkalescence of the blood must be altered in the opposite direction. When the stomach is empty, there is little if any acid there then. When food is taken, the quantity of acid begins to increase, and gradually

reaches the greatest amount poured out; and then by absorption, or by escape through the pylorus, the quantity of acid begins to decrease, until the stomach is again empty. If, then, food is taken twice daily, the acidity of the stomach might be represented by the following curved line :—



The amount of curvature, and the times when the highest limits are reached, must be determined by direct experiment. All that at present can be said is, that the acidity of the stomach is least before food, and greatest soon after food.

For the purpose of keeping up a constant supply of at least one inorganic acid, man has been led at all times, and in all circumstances, to seek for salt as necessary to his existence. What the influence of chloride of sodium undecomposed, and of other salts, is on the solubility of albumen or starch, has not yet been sufficiently determined.

The next most important constituent of the gastric juice, after water and acid, is the albuminous substance. Its exact nature is not known. I cannot consider it as epithelium. It is far more likely to be a substance like diastase; not albumen, not epithelium, but a peculiar albuminous substance. Its exact composition is not known; but probably it is a substance undergoing changes which it can communicate to other contiguous substances. It was precipitated by weak alcohol from infusion of pig's stomach by Wasmann, and he called it pepsin; it requires re-examination.

The non-albuminous organic substances in the gastric juice are the organic acids and fatty substance; the latter

probably exists in a very small quantity. This complex gastric juice cannot act precisely in the same way on two classes of substances so very different as nitrogenized and non-nitrogenized food.

Firstly. *The Action of the Gastric Juice on the Nitrogenized substances in the Food.*—The fine state of division of the food, the smallness of its amount, the constant muscular motion of the stomach, and the temperature of the body—these all assist the solution of fibrin, albumen, or casein. Strong acids very easily dissolve these albuminous substances; and the dilute acid ofthe stomach, in consequence, perhaps, of some influence of the nitrogenized pepsin, or animal diastase (?), is made to act as energetically on the albuminous food as strong acid would do. In this action there is nothing vital; it takes place as well out of the body as in it. The elements of the albumen cannot be converted so as to form water, salts, sugar, or fat. There is no formation of incipient albumen. If you please to call solution, reduction or combination of water with the substance dissolved, you may say the albumen is reduced; to me it is far more simple and quite as comprehensible to speak of it as a process of solution, and as nothing else.

Secondly. *On the Action of the Gastric Juice on Non-Nitrogenized Food, Starch, Sugar, Fat, &c.*—Starch is perfectly insoluble in water and in dilute acids, but by the action of dilute acid, it easily undergoes a change, by which it is rendered soluble. The action of strong sulphuric acid on starch, and the formation of sugar thereby, is probably well known to you already; but there is no strong acid in the stomach.

The relation of starch to British gum, or dextrine, or, as it has been called, soluble starch, is also well known to you. There are many ways of changing the insoluble starch into soluble dextrine. One very perfect method has been prac-

tised in France, of treating the starch, at the temperature of 100^0 Fahr., with dilute hydrochloric or oxalic acid, and thus dextrine is readily formed. There is but little doubt that the temperature and dilute hydrochloric acid in the stomach effect the same conversion as you see has been effected in this flask; further action of the acid and heat converts the dextrine into sugar. It has been said that the starch is acted on by the alkaline saliva, but directly the saliva reaches the stomach, it must be neutralized by the gastric juice.

On the Action of Gastric Juice on Sugar.—The ready solubility of sugar in water requires no illustration. A portion of the sugar which is taken as such, or is formed from starch in the stomach, without doubt passes into the blood as sugar; but it appears to me highly probable, from Fremy's experiments, that a portion is changed into some of the vegetable acids. The acetic and lactic acids may thus be formed, and these, in part, perhaps, may become lactates and acetates of soda in the blood. And we know, from direct experiment, what happens to vegetable acid salts injected into the blood, or taken into the stomach; they are oxydized or burnt, giving heat and carbonic acid salts, which pass off in the urine. All free vegetable acids are probably changed partly into carbonic acid in the blood. Thus, then, probably the progress of a grain of starch may be traced. It forms, first, dextrine; secondly, sugar; thirdly, vegetable acid; fourthly, carbonic acid.

The ascending conversion of sugar into albumen cannot be admitted until it is proved. There is not an experiment which renders such a change probable.

The descending conversion of sugar into fat, a substance also containing no nitrogen,—that is, of one kind of non-nitrogenized substance into another kind belonging to the same class,—is most fully proved. Bees fed on crystallized

sugar made wax, and animals form more fat than the food they are fed with contains; but this fat is not formed in the stomach, and therefore does not concern us now. The changes take place in the minute textures of the body, and not in the stomach.

On the Action of the Gastric Juice on Fat and Oil.—How is fat made soluble at the temperature of the body? Is oil absorbed? The following is an experiment by Tiedemann and Gmelin. A dog was fed for four days on butter; three hours after the last meal he was killed.

a. Stomach contained butter, and the contents were very acid. *b.* Small intestine contained butter and bile, and was acid strongly. *c.* The cæcum contained butter. *d.* The rectum contained butter. *e.* The chyle was very milky, and cleared with ether. *f.* The blood of the vena cava inferior contained much fat. *g.* The urine was thick; filtered, butter soluble in alcohol was left on the filter. They add: "One of our pupils, who likes fat, has frequently found it in his urine." This has also sometimes been observed after cod-liver oil. Oil and albumen form an emulsion slightly soluble in water; but the pancreatic fluid and the bile are generally considered as the agents which make the fat and oil of the food soluble.

Lastly, the phosphates and sulphates of soda, and the chloride of sodium, are soluble in water. The earthy phosphates are dissolved by the hydrochloric acid. Even silica, in minute quantity, is contained, dissolved in water, and hence these salts pass into the blood.

I may sum up the conclusions of this and of my former lecture thus. Each meal may be separated into these four classes of constituents—albuminous substances; non-nitrogenous organic substances, as starch, sugar, alcohol, fat; salts; water. Digestion of any meal is the solution in the stomach of these different substances, and their absorption from the bowels.

The formation of the gastric juice is certainly no chemical process, but in its action is entirely chemical, though it is aided by the motion which the muscular coat of the stomach produces ; and muscular contraction is as distinct as sensation from chemical action. Nor can the absorption of the dissolved substances be altogether considered as a vital action. It is certainly subject to the laws of the diffusion of one liquid into another, to the laws of endosmosis, or diffusion through a membrane, and to capillary action. This subject alone might occupy me for many lectures ; it belongs more to my colleague the lecturer on physiology than to me. I pass on, therefore, to the next subject, the formation of blood from the absorbed food, the assimilation of the food to the blood. How is it that the blood keeps its composition ? The food is dissolved, and then absorbed, and the blood somehow makes it into blood ; somehow preserves its own composition. What, then, is this blood chemically ? how does it differ from food in composition ? This will be the subject of my next lecture.

LECTURE III.

ON THE BLOOD.

I STATED to you, in my last lecture, that the clearest idea of digestion is obtained by dividing it into solution and absorption, and by considering the assimilation, or making of the food into blood,—sanguification, that is,—as a separate and distinct process from digestion. I pointed out that reduction meant little, if anything, more than solution, that conversion of one class of substances into another class—viz., water into salts, (ashes;) salts into non-nitrogenous organic substances; non-nitrogenous organic substances into nitrogenous organic substances, so far as we know, never takes place in the human body. A change of one substance into a higher compound rarely occurs, but everywhere, in the stomach and system lower compounds are each moment forming, and this chiefly for the purpose of solution. Dissolved thus, they can pass into the blood, and when there, they are made like blood; and when the blood passes into the different organs of the body, each organ assimilates the blood, or makes the blood like itself, taking out of the blood those substances which are necessary for its support. After being used, in order to make room for new substance in the different organs, the old substance must be removed—

must be re-dissolved, and re-admitted into the blood, to be thrown off by the excretions.

It will be the object of my lecture to-day to attempt to give you a general view of the relation of the blood to the food, to the body, and to the urine; to show you that the blood, like all vegetable or animal food, consists of water, nitrogenized substances, non-nitrogenized substances, and salts, which, when burnt, give ashes.

Any single analysis of the blood is of no value quantitatively. It serves well, however, to show you what kind of substances enter into its composition. There is not, nor, indeed, can there be, any standard analysis of blood, to which all others may be referred. The blood has been called an internal atmosphere, and in its constant momentary variations, in its unceasing change, it may well be compared to the atmosphere. Each moment its composition, as a whole, is changing. Each respiration produces its change on the blood. Each time food is taken, a great change in the blood must occur. You cannot add a pound or two of matter to the blood without changing it. Each action of a muscle, or nutrition of any part of the body, must take something from the blood, and thus change its composition. So, also, each excretion from the blood must effect its peculiar changes on that blood, out of which each excretion is taken. These are the broad outlines of the causes of the changes of the blood. The water is always changing. The nitrogenized and unnitrogenized substances are always varying in amount; even the salts, even the alkalescence of the blood, is in a perpetual state of variation; at no two moments of the day is it the same. The quantity, then, of the various substances present in the blood is constantly changing, and the variations in the state of health require to be far more minutely studied than they have been, before deductions as to the variations in disease can be safely trusted.

I shall give you the best general idea of the quality or nature of the substances in the blood by showing you here the method I follow in making an analysis of the fluid. It is very closely similar to the process followed by M. Andral, and therefore the results obtained are comparable with the results he has given, and this alone to the physician is a matter of importance.

A light bottle, like this one, with a wide mouth, should be taken, the weight of which is known, and it must be perfectly clean and dry. This bottle should be accurately filled with blood, and, if possible, it will be better if the blood be allowed to pass directly from the arm of the patient into the bottle. Allow the bottle to stand at rest until the blood be perfectly coagulated. Ascertain the weight of the bottle and its contents, and if moderate care be taken, the serum, perfectly clear, may be thus poured off from the coagulum into a light, dry, and clean basin, the weight of which is known. Now the weight of the serum—at least, of that portion separated from the clot—is thus easily ascertainable, either by weighing the bottle from which it has been decanted, or by weighing the basin into which it has been put, or by both methods.

I have here some blood which I yesterday analyzed, and here are the separate constituents; and the steps I have been describing gave the following numbers:—

The bottle weighed 3021 grains, when filled with blood the weight was 4493 grains; consequently the weight of blood experimented on was 1472 grains. By the means mentioned, the serum poured off was found to weigh 490.76 grains. The serum was evaporated to dryness; and in order to ensure a perfectly dry residue, the basin should be placed under the receiver of an air-pump, over sulphuric acid, until it ceases to lose weight. When perfectly dry, the basin and solids of serum weighed 1,042.30 grains; the weight of the

basin was 1,000 grains; so the solids of serum were 42.30 grains, and the water in which these solids were dissolved was 448.46 grains.

The quantity of fibrin in the blood is next to be determined, and for this purpose the blood may be beaten during coagulation with a rod. By this method some fibrin is always left behind. I think it better to place the coagulum on a piece of well-washed linen, then to tie it up accurately and tightly, and wash it in distilled water until it is colourless. The blood globules pass through the linen easily, and if the washing be continued long enough, the fibrin will be perfectly white; after this, dry the fibrin, first in a water-bath, then in vacuo, over sulphuric acid, and weigh it. In this case the coagulum and residue of the serum weighed nearly 981 grains. The fibrin, which you see here, was found to be 5.909 grains. The remaining steps of this analysis are these: the 981 grains consisted of globules, fibrin, and residue of the serum. Deducting the fibrin, we have 975.1 grains, to which a considerable quantity of distilled water has been added. The whole is to be evaporated to dryness in a water-bath, thus, and afterwards in vacuo; the dry residue is to be weighed, and it was found in this instance to be 227.5 grains; the quantity of water, therefore, in the clot and residue of serum, was 747.6 grains. In the commencement of the analysis, 448.46 grains of water contained 42.30 grains of solids of serum, consequently 747.6 grains of water will contain 70.52 grains of solids of serum. This amount must be deducted from the whole amount of dry residue of the coagulum—that is, from 227.5 grains, and then the weight of the dry blood-globules will be left, 227.5 grains — 70.52 grains = 156.98 grains, blood-globules. The analysis may be seen in a tabular form, thus—

			Grains.
		Blood+bottle	4493
		bottle	3021
		Blood	1472
Blood+bottle ...	=4493	Basin+serum ...	= 995.86
Blood+bottle—serum	=4002	Basin	= 505.10
		Serum	= 490.76
Serum ...	= 491	Solids of serum	= 42.30
		Water ...	= 448.46

Fibrine	= 5.909 = 4.01 per 1000 blood.
Blood+bottle—serum	=4002
bottle	=3021
Dry blood globules, residue of serum, fibrin+water	= 981
Fibrin	= 5.9
Blood globules, dry serum, water	= 975.1
On evaporation, dry residue	= 227.5
∴ Water	= 747.6
This water must contain solids of serum . .	= 70.52
∴ Blood globules = 227.5 grs. − 70.52 grs. ...	= 156.98 = 106.7 per 1000
Solids of serum = 70.52 grs. + 42.30 grs. ...	= 112.82 = 76.6 per 1000

Hence in 1000 parts of blood—		Andral, healthy blood.
Fibrin	4.0	3.00
Blood-globules	106.6	127.00
Solids of serum	76.6	80.00
Total solid residue	187.3	200.00
Water	812.7	800.00

Where blood is plentiful, it is well to have a second bottle filled and evaporated to dryness in a water-bath, and in vacuo, and thus the total solid residue will be directly obtained. Then treat this with ether frequently, until the ether ceases to dissolve out any fat. Mix the etherial solutions, evaporate and weigh, and then you have directly the amount of fat. Then ignite the residue of the blood, and thus an approximation to the amount of ashes can be obtained. But no problem is more difficult than to determine accurately the saline constituents of healthy blood.

Thus, then, we have in the blood, albumen, fibrin, blood-globules—all albuminous substances ; fat, a non-nitrogenous organic substance ; salts ; and water.

The albumen, fat, ashes, and water, are the same as existed in the food. The fibrin and the blood-globules are the constituents of the blood, which do not exist in the food. It is the production of these by the blood out of the food, to which the term assimilation must chiefly be applied. The chemistry of this assimilation—that is, the chemistry of the formation of fibrin and blood-globules—is at present almost entirely unknown to us, and therefore it is well called a vital process, and belongs as yet to physiology, and not to chemistry.

In disease, the constituents of the blood undergo various and great alterations in their amount ; sometimes some constituent is found to be much increased, at other times much diminished. Tables illustrating these changes you will find in Andral's work on the blood. For the purpose of making the effect of disease very evident, I have chosen out from that work the following analyses, which present the most striking variations, and you will see therefrom how an analysis of the food might assist your diagnosis in a difficult case.

BLOOD.	Health.	Variation in Disease.	Rheumatism.	Fever.	Anæmia.	Cerebral Congestion.	Bright's Disease.
Fibrin	3	10½ to 1	10	.9	3.5	2.7	3.2
Globules	117	185 to 21	101	93.1	38.5	152.3	82.
Solids of serum	80	114 to 57	90	86.0	89	105.	74.8
Water	800	915 to 725	799	820.	869	740.	850.
	1000		1000	1000	1000	1000	1000

In this table, the contrast, in the amount of fibrin, between the blood in fever and rheumatism is very marked.

It also shows well how high the fibrin is both in anæmia and in Bright's disease; and hence the liability to acute or subacute inflammations in these diseases. In both, although the amount of blood-globules is greatly diminished, the fibrin is increased. In Bright's disease the decrease of the albumen of the serum is very evident also.

It has lately been proved by Dr. Garrod, that in gouty patients, urate of soda is contained in the serum of the blood. This, although long suspected from the occurrence of deposits of this salt, round, and in the joints of some patients, had never been proved to the satisfaction of the chemist. The method I use for obtaining the demonstration of uric acid in the blood is by no means difficult. I have here the blood drawn from two patients in the hospital. The clear serum is poured into a basin, and evaporated at 212° to perfect dryness. The mass is to be reduced to the finest possible powder, and then treated at the temperature of 100° F., with distilled water for an hour; by this means everything soluble in the residuum is obtained in solution, and the urate of soda being soluble, is dissolved out. The solution is then to be evaporated to a very small bulk, and strong acetic acid should be added. Acetate of soda is thus formed, and uric acid is set free; and in the course of a few, or many hours, according to the quantity of uric acid and the dilution of the mother liquor, the uric acid crystallizes out, as you see it here, on the sides or bottom of the tube. To the eye, the crystals look like small grains of cayenne pepper; through the microscope they look like some uric acid crystals from the urine; and if a few of them are taken, treated, thus, with nitric acid and ammonia in the usual way, the diagnostic pink reaction is obtained. Here in this basin the reaction is, you see, most clear and unequivocal. That these were gouty patients is certain, from the following notes of their cases:—

J. J——, aged fifty, (married, and the father of five children, the eldest seventeen,) an engineer, admitted by me, Jan. 1st, 1849; never had rheumatic fever, but has been subject to gout for the last sixteen years. The first attack took place during the night, with great pain in the great toe of the right foot, as though a heavy weight had fallen on it; the attack lasted some days, and he had no return for several months. The attacks have been more frequent during the last ten years, from which time the small joints of the hands and feet have always been enlarged, and very stiff. He states that he has taken large quantities of Blair's pills, and also a great deal of colchicum, in small doses—viz. from fifteen to twenty drops.

For the last eight weeks he has never been free from pain; it has existed chiefly in the hands, knees, and feet, which are swollen and stiff, and in many places he has chalky deposits; he has pain also in the hips and elbows. On the external canthus of each ear there is a chalky deposit, which gives beautiful long acicular crystals, which become pink when heated with nitric acid. Pulse 108, strong; heart sounds healthy; passes plenty of water; bowels, regular; the pain is worse the last four days; tongue coated; perspires freely, and cannot sleep for pain. Has never been a free drinker, and does not belong to a gouty family. Bled to fourteen ounces.

Jan. 2nd.—The blood in the large vessel was very much buffed and cupped; buff above one-eighth of an inch thick. Blood in the small vessels not cupped; serum slightly alkaline; specific gravity, 1029.1; uric acid, in plenty, was obtained by the above process. The urine made in twenty-four hours, commencing from one hour previous to the bleeding, was about two and a half pints, specific gravity, 1018. It was thick, from urate of ammonia. Acidity for every 1000 grains of urine equalled nearly a grain and a

half of carbonate of soda, (16.70 measures.) The urine gave a considerable coagulum with heat and acid, proving that albumen was present in it. 2036.0 grains of urine, with five drachms of strong acetic acid, gave uric acid = 1.20 grs. = 0.59 grs. per 1000 grains urine. With salines and colchicum all fever and pain subsided. He went out, relieved, on the 6th of March, the urine being still albuminous.

The second case was that of a carpenter, W. L——, aged thirty-seven, admitted by me, Dec. 17th, 1848; has been subject to attacks of rheumatic gout for the last seven years, for which he has taken large quantities of colchicum. The attacks have occurred more frequently the last four years, and from that time, the joints of the hands, knees, and feet, have been enlarged. When he felt a paroxysm coming on, he was in the habit of taking colchicum, in doses of two drachms of wine, twice a day, and frequently as much as an ounce of the wine in three days, with various effects, sometimes producing violent purging, at other times, great sickness. During the last five months, the attacks have followed each other in such quick succession, that he has had scarcely any intervals of ease, whereas, before, the fits lasted but a few days. The joints, also, have become more enlarged and stiff. There is a large chalky mass in the left elbow, and a little speck in the ears. He makes very little water, and it contains no albumen, but is high coloured. Bowels act daily; tongue furred; pulse bounding. Bled to eight ounces; salines and colchicum.

28th.—The blood in the small vessels was buffed and cupped; the large vessel had no cupping or buffing; the serum was slightly alkaline, specific gravity 1029.8. It gave slight but perfectly distinct proof of the presence of uric acid.

The urine passed from the time of the bleeding to twenty-four hours after was rather less than a quart, clear, specific

gravity = 1016.0. It gave no deposit on standing. The acidity of 1000 grains of urine was equal to rather more than a grain of carbonate of soda, (13.78 measures).

2032.0 grains of urine, mixed with four drachms of hydrochloric acid, uric acid = 0.35 grains = 0.17 grain, per 1000 grains of urine.

He went out on the 25th of January, relieved.

Dr. Garrod has very lately detected in the blood a substance which crystallizes in microscopic octahedral crystals. From its reactions he considers this to be oxalate of lime. But this, and all other statements founded on microscopic chemistry alone, must be received with great caution.—*Med. Chir. Trans.*, 1849.

Lastly, in the blood of the ox, a substance resembling hippuric acid, in crystalline form and chemical reactions, has just been discovered.—*Comptes Rendus*, Dec. 24th, 1849.

LECTURE IV.

ON CALCULI.

As in the food and in the blood, so even in the urine, we have the four classes of constituents,—water, nitrogenized organic matters, non-nitrogenized organic matters and salts; though the forms in which the elements occur in the urine are different from what they are in the food or in the blood.

I shall not attempt to give you a process for the complete analysis of the urine; for determining the qualities and quantities of each and of all the substances of which the urine consists; but in order to bring before you some of the most important constituents which occur in that excretion, I shall commence these lectures with those natural analyses or separations of some of the principles of the urine which constitute the different kinds of stone. By this method the most important components of the urine will be fixed, I hope distinctly, in your minds, and you will be less likely to be confused in the view of the urine which I shall give you in the following lectures.

The substances which are precipitated from the urine to form calculi, are separated, because, from some cause, the water in the urine becomes incapable of dissolving them. You never heard of urea or sugar, salt or albumen, forming

a calculus, because nothing can pass off in the urine which can make the water lose its property of dissolving them. Very different is the case, for example, with uric acid or earthy phosphates.

The causes of the formation of calculi may be shortly and generally stated thus:—There may be too much of any substance for the water to dissolve; or too little of the water for the solution of the substance; or the solvent property of the water may be lost in consequence of some third substance being present in it, or being absent from it. By each of these methods different natural analyses, so to speak, of the urine may be made, and in each of these ways different urinary calculi may be formed.

My object now is to show you that there are different substances separated from the urine, and of what they consist, and to fix their properties and names in your minds.

The first great division is made by testing different calculi by heat, and this ought always to be the first test which you should use. For this great difference exists between substances that are separated from the urine—some of them can be burnt, and others cannot burn away. Not very long since a muffle and mint furnace were used. Now a platinum spoon, like this, and a spirit-lamp are employed, and occasionally a blow-pipe is required. Thus we separate all calculi into two classes—those destroyed by heat, and those not destroyed by heat.

The calculus is next to be tested by dilute mineral acids; if it be destroyed by heat, then nitric acid is to be used; if it be not destroyed by heat, hydrochloric acid is best.

First, of the calculi destroyed by heat. The least possible quantity of the calculus in powder should be placed in a small basin, or still better, in a watch-glass, thus. The action of nitric acid on these substances destroyed by heat produces two results,—either they are reddened when the acid is evaporated

to dryness, or they are not so. If the first be the case, if a red colour be produced, such colour is always increased in intensity by the addition of the vapour of ammonia. If this reaction occurs, the calculus is uric acid, or urate of ammonia, these being the only two bodies which are acted on in this manner by heat and nitric acid. We can distinguish urate of ammonia from uric acid by the action of carbonate of potash, which dissolves both, but when heated, evolves carbonate of ammonia from the urate of ammonia, but none from the uric acid. The evolution of the carbonate of ammonia can be detected by the smell, or by the action on red test-paper wetted. There is a further distinction between these two substances, which it is useful to know. Urate of ammonia is soluble in hot water, and is precipitated on the addition of any acid. A little of the calculus is scraped off, and placed with a few drops of water in a watch-glass, and heated over a spirit-lamp, thus. The powdered calculus readily dissolves, and if the solution be strong, there is a precipitation of urate of ammonia as the solution cools. If this do not occur, then a precipitate is readily obtained on the addition of a drop or two of hydrochloric, or some other acid. On the other hand, uric acid is not acted on by warm distilled water; but it is readily dissolved by dilute caustic potash, and it is reprecipitated by any acid, and the microscope can detect the crystalline form of the precipitate.

The combustible substances not made red by nitric acid are two in number, both of rare occurrence, one cystine or cystic oxide; this, when treated with nitric acid, and evaporated to dryness, gives a black residue. The calculus is soluble in ammonia, and crystallizes very beautifully from its solution on the evaporation of the liquid. If this reaction does not occur, the calculus may be fibrin, which is not easily soluble in ammonia, and when it is dissolved does not crystallize on evaporation.

Cystic oxide contains as much as twenty-five per cent. of sulphur, and on this fact the following test is founded. Take a small portion of the calculus, which will have a waxy crystalline appearance; place it in a test-tube, thus, and add caustic potash and a little distilled water, and boil the fluid for a few minutes; then add one drop of a solution of acetate of lead, and in a few minutes, the heat being continued, a jet-black colour is formed, as you see, by the production of sulphuret of lead.

Secondly, of the calculi not destroyed by heat. Oxalate of lime, though not destroyed, undergoes a change when heated. Oxygen is taken up by the carbon, and instead of two equivalents of carbon and three of oxygen, we have two carbon and four oxygen, thus, $C_2\ O_3$ gives $C_2\ O_4$. As the heat is continued, this carbonic acid is partly or wholly driven off, and quick lime remains.

The appearance of this calculus is very frequently conclusive as to its composition. It is often exceedingly rough and nodulated, and as it thus resembles a mulberry it has been called the mulberry calculus. On heating it, it crackles and flies about, and for this reason it is well to cover the platinum spoon, lest the substance be lost. At a red heat it chars, blackens, and continues burning for some moments after the heat is removed. Into two test-tubes put a little water, and in one some of the burnt calculus, and in the other a portion of the unburnt calculus; add a drop of hydrochloric acid to each; in the test-tube containing the unburnt calculus no change is produced, while from the other test-tube carbonic acid is evolved in plenty, as you may see here.

The calculus is dissolved by the acid, and its solution, when carefully neutralized, is precipitated by oxalate of ammonia, oxalate of lime being again produced. Another special test for oxalate of lime is the formation of oxalate of

silver. But the only infallible test is the decomposition of the oxalic acid into equal volumes of carbonic acid and carbonic oxide gasses. For this purpose the calculus must be reduced to powder, and mixed in a small tube retort, with strong sulphuric acid, and then heated carefully. The gasses must be collected over mercury; on examination, one half should be absorbed by caustic potash, whilst the other half should burn, and after combustion it also should be entirely absorbed by the alkali.

Another kind of calculus indestructible by heat consists of the earthy phosphates. Of this there are two varieties—the phosphate of lime and the phosphate of ammonia and magnesia. They constantly occur, mechanically mixed in the same calculus, which, then, has received the name of the fusible calculus. The phosphate of lime is precipitated alone chiefly in consequence of indigestion; the phosphate of ammonia and magnesia forms a deposit most frequently in consequence of diseased bladder. Neither phosphate is destroyed by heat; both are soluble in acids, without effervescence. A portion of the the calculus composed of either of these varieties, after being heated, is placed in a watch-glass; water is then added, and afterwards a drop of hydrochloric acid: the absence of effervescence will plainly be seen. The solution in acid, on the addition of an excess of ammonia, gives a gelatinous precipitate. To the naked eye, the precipitate of phosphate of lime and phosphate of ammonia and magnesia are very similar; but when seen through a microscope the difference is very striking—the one is amorphous; the other, highly crystalline. If no microscope is at hand, a further step must be taken, in order to determine which of these two phosphates is present. The method is to see whether the calculus is made more or less fusible, by adding to it bone-earth or phosphate of lime. The phosphate of ammonia and magnesia, with half its bulk of phosphate of lime, is fusible

by the blow-pipe. The phosphate of lime becomes fusible only when twice its bulk of phosphate of ammonia and magnesia is added to it.

Lastly, silica calculus differs, chiefly in negative properties, from all the preceding compounds. It is not destroyed by heat; it is not easily acted on by alkalies or acids. It requires to be fused with carbonate of soda, so as to form silicate of soda, which is soluble in water, and precipitable by hydrochloric acid. It occurs so rarely, that it scarcely requires to be mentioned here.

The accompanying table (p. 40) shows these reactions most clearly; and, by it, the properties and names of the different kinds of calculi will most easily be learned.

The great difference between these reactions will be sufficient evidence to you, that very different causes must have been in action when these substances were deposited from the urine. Some of the substances, you see, are soluble in acids, and precipitated by alkalies; others are soluble in alkalies, and precipitable by acids. For the formation of these two different kinds of calculi, two opposite conditions of urine must have occurred. The precipitation of uric acid invariably is caused by excessive acidity of the urine, and by nothing else, and this acidity, in a less degree, sometimes causes a deposit of urate of ammonia. The precipitation of phosphate of lime, and phosphate of ammonia and magnesia, is caused by diminution of the acidity of the urine, or by its becoming ammoniacal.

Oxalate of lime, cystine, silica, and, frequently, urate of ammonia, form calculi, because of their difficult solubility in healthy urine, or because the quantity in which they are present is too great for their solution in the proportion of water that is excreted at the same time.

In all calculi, whatever their nature may be, small quantities of fat and mucus can be discovered, and when recently

removed from the bladder, all calculi are saturated with moisture, containing minute quantities of the salts of the urine. So that even in calculi we may find those four classes of substances which, in nature, are so rarely separated :—1st, water ; 2nd, salts, or ashes ; 3rd, fat, or non-nitrogenized organic matter ; 4th, mucus, or nitrogenized organic matter. Thus, we find the same classes of substances as enter into the composition of vegetables, milk, or meat ; but here how different the relative proportions in which they occur ; how unfitted, consequently, are calculi for the support of any other animals than the monads and vibrios which even here can find their nourishment, as long as there is sufficient water for them.

TABLE FOR EXAMINING URINARY CALCULI.

1. *By Heat.*	2. *By Acids.*	3. *By Alkalies.*	*Nature of the Calculus.*	*Special Tests.*
Destroyed by heat.	With nitric acid, red.	Soluble in carbonate of potash, evolving ammonia.	Urate of ammonia.	Soluble in water, when boiled. Solution in water, with a few drops of ammonia, evaporated, crystallizes in needles, (microscopic.)
		Soluble in carbonate of potash, evolving no ammonia.	Uric acid.	Solution in caustic, ammonia or potash, on the addition of an excess of acid, crystallizes in angular crystals. Not soluble in water.
	With nitric acid, not red.	In ammonia soluble, crystallizing when evaporated.	Cystine.	Soluble in strong caustic potash; the solution, boiled for a few moments, on the addition of a drop of dilute acetate of lead, gives sulphuret of lead.
		In ammonia with difficulty soluble, not crystallizing.	Fibrin.	With nitric acid becomes directly yellow. Solution in caustic potash precipitable by acetic acid in amorphous form.
Not destroyed by heat.	With hydrochloric acid soluble; before heating effervesces.	Solution in acid, when neutralized, gives a precipitate with carbonated alkalies and oxalate of ammonia.	Carbonate of lime.	Soluble in dilute acetic acid, with effervescence.
	With hydrochloric acid soluble; after heating effervesces.	Solution in acid, when neutralized, gives a precipitate with carbonated alkalies and oxalate of ammonia.	Oxalate of lime.	Insoluble in acetic acid. Boiled with carbonate of soda, oxalate of soda is dissolved, precipitated by chloride of calcium.
	With hydrochloric acid soluble without effervescence with or without heat.	Solution in acid, with excess of ammonia, gives a crystalline precipitate.	Phosphate of ammonia and magnesia.	With half its bulk of phosphate of lime, (bone-earth,) is very fusible by the blow-pipe.
		Solution in acid, with excess of ammonia, gives an amorphous precipitate.	Phosphate of lime.	With twice its bulk of phosphate of ammonia and magnesia, is very fusible by the blow-pipe.
Not acted on.	Not acted on.	Not acted on.	Silica.	Fused with twice its bulk of carbonate of soda, forms glass, soluble in water, precipitable by hydrochloric acid.

LECTURE V.

ON THE QUANTITY AND ACIDITY OF THE URINE.

In my last lecture I showed you some of the substances which can be separated from the urine; some from a deficiency of water; some from an excess of acidity, or alkalescence of the urine. We will now consider the quantity and acidity of the urine, leaving the alkalescence for a future lecture.

The quantity of urine usually varies with the quantity of water drank. It varies inversely also with the quantity of water thrown out by the skin and the bowels. Thus, in summer the urine is less than in winter, and in the colliquative perspiration of phthisis, the amount of urine excreted is very small; and thence, partly, the deposits of urate of ammonia in the urine which are then found to occur. So, also, the suppression of the urine in cholera may partly result from the excessive excretion of fluid by the intestines.

If about fifty ounces of fluid are taken in twenty-four hours, the average quantity of urine in health, during winter, thrown out, will be forty-four ounces daily. On full diet, with moderate exercise, the specific gravity of such urine will be about 1021—that is, a bottle which, when filled with distilled water, at a temperature of 60° Fahr., weighs

1000 grains, when filled with urine at the same temperature, will weigh 1021 grains.

The secretion of urine appears to be more active early after breakfast than late after dinner. During sleep—the quantity of fluid drunk daily being fifty ounces—the urine may be secreted at the rate of two ounces an hour, specific gravity=1019; the first three hours after breakfast, at the rate of three ounces an hour, specific gravity=1016; during three hours of the day when most active exercise is taken, not an ounce and a half an hour, specific gravity, 1024; and as little during three hours after a full meal in the evening. The most rapid secretion of urine I know of, except in cases of hysteria, follows action of the bowels after breakfast: the bladder, having been perfectly emptied, in three minutes contained an ounce of urine, specific gravity 1004.6.

If a diet of animal food only is taken, the quantity of liquid being fifty ounces, then the quantity of urine is not less than when a mixed diet is taken, but the specific gravity is higher.

If an excess of exercise is taken, the quantity of urine is diminished, and the specific gravity is increased; and the highest specific gravity occurs when the exercise is greatest, and the diet only animal food.

The specific gravity of the urine varies of course with the amount of solid substances dissolved in its water. In this it resembles any other solution. For instance, if a large quantity of sugar be dissolved in 1000 grains of water, the weight of the liquid will be much increased. If but little sugar be dissolved in the same quantity of water, the weight will be but little increased. The healthy urine contains urates, sulphates, chlorides, phosphates, urea, in constantly varying proportions; these are some of the substances in solution which cause an increase in the weight of the urine as compared with the weight of an equal bulk of distilled

water. The less of these substances in the urine, the lower the specific gravity will be. The more of these substances in solution, the higher will the specific gravity be.

The following table, from the 27th volume of the *Medico-Chirurgical Transactions*, will make the effect of food and exercise on the quantity and specific gravity of the urine more evident to you :—

	Ounces.	Specific Gravity.
28 days, average daily quantity and specific gravity	$43\frac{3}{4}$	1020.75
Diet.		
8 days, meat diet only	$43\frac{3}{4}$	1022.41
25 days, mixed diet	$43\frac{3}{4}$	1020.32
Exercise.		
5 days, exercise in great excess	$41\frac{3}{4}$	1023.20
23 days, exercise moderate	44	1020.11
Diet and Exercise.		
3 days, exercise in excess, diet animal	$42\frac{1}{2}$	1023.57
2 days, exercise in excess, diet mixed	$40\frac{1}{2}$	1022.63
5 days, exercise moderate, diet animal	$44\frac{1}{2}$	1020.51
18 days, exercise moderate, diet mixed	$44\frac{1}{2}$	1020.10

It will be seen, that while exercise reduces the quantity of urine secreted, and as a consequence increases the specific gravity of that fluid, diet, whether animal or mixed, has but little, if any, influence over the quantity of urine secreted; while, if the quantity of urine be the same, the specific gravity is increased by an animal as compared with a mixed diet.

Tables have been constructed professing to tell how much solid matter is contained in urine of any specific gravity. It is said, that by taking the specific gravity, and referring to the table, the quantity of solid matter may be immediately

determined. If the urine were simply a solution of one substance,—as, for example, urea in distilled water,—such tables could be made to give the truth; but when many different substances are dissolved in water, no tables can be trustworthy. A small quantity of one substance may increase the bulk of the urine more than a larger quantity of another substance, or equal quantities of different substances may increase the bulk of equal quantities of water in which they are dissolved to a very different degree; so that the solid residue in each might be equal, while the specific gravities of the solutions might be different. Experiment proves this, in the case of the urine. There is no short road to any accurate results. The acid urine must be carefully evaporated at a very low temperature in the vacuum of the air-pump, over sulphuric acid, until on being weighed and re-weighed, it ceases to lose weight. If the urine be not acid, the result will be worthless. The following experiments were thus made on the specific gravity and solid residue of the urine before and after dinner. About 500 grains of urine were in each case evaporated.

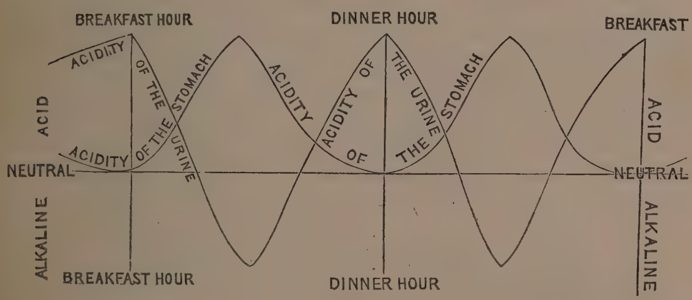
	Specific gravity.	Solid residue per 1000 grs. of urine.			
Before dinner,	1028.0	... found 67.03	grs.	by table 65.2	grs.
After dinner,	1028.5	... „ 66.59	„	„ 58.5	„
Before dinner,	1028.2	... „ 64.77	„	„ 65.0	„
After dinner,	1034.3	.. „ 84.65	„	„ 79.0	„
Before dinner,	1024.7	.. „ 60.77	„	„ 58.0	„
After dinner,	1024.8	... „ 64.61	„	„ 58.0	„
Before dinner,	1024.8	.. „ 56.67	„	„ 58.0	„

These experiments show that the quantity of solid contents cannot be determined by taking the specific gravity.

Not only does the quantity of urine and the substances dissolved in it vary, but each substance has its peculiar variations; so that there is no one analysis of the urine as to the quantities of different substances present in it which

can give any fixed standard. The quantitative analysis which is true for one hour of the day may be quite different from the quantitative analysis of another hour. The quantities found when one diet or amount of exercise is taken are not true for a different diet, or when more or less exercise is taken. If this occurs in health, still more is it true in disease. No single analysis can give the whole truth. The variations which take place in health must first be determined, and then, and not till then, we can judge what variations are produced by disease.

I shall first dwell on the most important variation in the urine—namely, its acidity. The urine in health is generally acid, but the quantity of acid is continually changing, and this variation is one of health, and not, as has usually been stated, one of disease. You saw in a former lecture that the quantity of acid in the stomach continually varies, and in the urine corresponding alterations, equally remarkable, occur. The variations of the acidity of the stomach and urine in an extreme case may be represented by curved lines, as in the accompanying diagram :—



The method I employ for determining the relative amount of acidity of the urine is the following :—A long graduated

tube like this, accurately divided into 100 measures, is taken, and a test alkaline solution is prepared by heating pure carbonate of soda until it has lost all its moisture. In 144 measures of distilled water at 60° F. twelve grains of carbonate of soda are dissolved; each measure, therefore, contains the twelfth of a grain of carbonate of soda (dry and pure). For calculation it would be far better if each measure had contained one-twentieth of a grain of carbonate of soda.

A known weight, usually about 1000 grains of urine, is then heated to the temperature of 130° Fahr., and kept about this heat, and well stirred while the test-solution is dropped in thus, until the re-action on test-paper shows that the neutral point is reached. The number of measures used are then marked, and a simple calculation gives the quantity required to render 1000 grains of the urine neutral.

By this means I found that the acidity of the urine was always varying. It became most interesting to trace when the acidity was greatest, and when least, and to try and determine the causes of the variations.

From a multitude of experiments, I found that the urine passed the longest after food is generally the most acid; and that that passed when digestion is going on is three or four, or more, times less acid; and that sometimes, even in health, it is alkaline. The average of five days, breakfast being about nine, and dinner between six and seven, I found was this:—

	o'Clock.	Spec. Grav.	Per 1000 grs. of Urine.
Urine made between	1 & 2 ...	1025.0 ...	acidity, 13.07 measures.
" "	6 & 7 ...	1025.7 ...	.. 26.12 ...
" "	10 & 11 ...	1026.7 ...	.. 13.81 ...

If vegetable food only was taken, the acidity was as high

between six and seven; but between ten and eleven, and one and two, it was lower.

The third day of vegetable food, the following remarkable variations occurred:—

		Spec. Grav.	Per 1000 grs. of Urine.
Urine made at	10½ A.M.	1025.6	acidity, 12.67 measures.
"	9½ P.M.	1024·5	alkaline.
"	9½	1025.9	acidity, 26,31 "
"	10½	1014.8	alkaline.
"	6½ A.M.	1014.8	acidity, 7.88 "

If animal food only was taken, the urine, before food, was still always the most acid. After food it was also occasionally alkaline.

The first day of animal food only—

		Spec. Grav.	Per 1000 grs. of Urine.
Urine made at	9 A.M.	1023.3	acidity, 30.41 measures.
"	11	1015.3	alkaline.
"	1 P.M.	1022.2	alkaline.
"	6½	1023.9	acidity, 20.50 "
"	10½	1024.1	" 11.71 "

Three months after these experiments were made, I determined to trace more carefully the extent and time of the variations. For this purpose another test-solution was prepared, containing so much dilute sulphuric acid, that each measure of this solution should neutralize one measure of the test-alkaline solution. Thus I could determine the degree of alkalescence, as well as the degree of acidity. (For the diagrams, which represent the variations far better than words or figures can do, I must refer to the plates in the *Philosophical Transactions* for 1849, part ii.)

From these experiments, it appears that immediately before each meal the urine showed the highest degree of

acidity; and the water passed two, three, or more hours, after food, always showed a lower degree of acidity. The decrease was greatest three hours after breakfast, and five or six hours after dinner, when it reached the lowest point. The acidity of the urine then increased, until immediately before food, when it again reached the highest limit. If no food was taken, the acidity of the urine did not decrease, but remained nearly at the same degree of acidity for twelve hours; but, then, directly after food was taken, the acidity fell.

When animal food only was taken, the diminution of the acidity after food was more marked and more lasting than when a mixed diet was taken. The increase of acidity before food was hardly so great as when mixed diet was taken.

When vegetable food only was taken, the decrease in the acidity was not so great as when animal food was taken—that is, though the urine became neutral after food, it did not become highly alkaline. The increase in the acidity of the urine was higher before vegetable food than it was before animal food was taken.

Dilute sulphuric acid was taken in large doses, but it did not produce a very decided effect. Nine drachms of dilute sulphuric acid in three days slightly diminished the decrease in the acidity of the urine after food; but the acidity before food was very slightly, if at all, increased thereby.

By comparing the acidity of the whole quantity of urine passed in twenty-four hours for three days when no sulphuric acid was taken, with the acidity when nine drachms of dilute sulphuric acid were taken during three other days, the increase in the acidity, though not very marked, was decidedly apparent. The average quantity of carbonate of soda required to neutralize the whole of the urine made in twenty-

four hours, when no sulphuric acid was taken, was 15.39 grains. The average quantity required when sulphuric acid was taken, was 20.38 grains. There was but little difference in the quantity of urine made.

Tartaric acid, in large doses, produced a decided effect on the acidity of the urine. 354 grains of dry and pure tartaric acid, taken in three days, increased the acidity of the urine; but in that time it did not render the effect of digestion on the re-action of the urine less apparent than it was when no acid was taken.

Liquor potassæ, in large doses, produced a decided effect in diminishing the acidity of the urine; but it by no means renders the urine constantly and permanently alkaline. Its effect seems to pass away rapidly. An ounce of strong liquor potassæ, taken in three days, did not counteract or conceal the influence of digestion on the re-action of the urine.

Tartrate of potash produced a most decided and rapid effect on the acidity of the urine; 120 grains of pure dry tartrate of potash, dissolved in four ounces of distilled water, made the urine alkaline, in thirty-five minutes. In two hours the alkalescence had disappeared; but after the next meal, the effect of the tartrate of potash was again apparent. Ten drachms of tartrate of potash, in three days, produced but little, if any, effect on the acidity of the urine after it had been omitted for twenty-four hours.

The result of these experiments is, that the acidity of the urine is always changing, and that the changes depend on the state of the stomach. Whilst much acid is in the stomach, the acidity of the urine is increased, and usually it reaches its highest limit before food is again taken.

Animal food causes a greater oscillation than vegetable food does, and when no food is taken, the variation is very slight.

The influence of animal food in lessening the acidity of the urine, and the influence of vegetable food in increasing the acidity are in remarkable contrast. Possibly the effect of oil, starch, and sugar on the acidity of the urine may be separated, and the influence of each may be proved. That these three substances, after they are taken into the stomach, probably pass through various neutral and acid states before they become carbonic acid and water, I have already mentioned to you. The urine may from these substances receive many acids which, when animal food alone is taken, may be altogether absent. That vegetable acids partly pass off by the kidneys, the experiments with tartaric acid sufficiently prove.

The variations in the acidity of the urine indicate corresponding variations in the alkalescence of the blood. In both fluids, the variations are unceasing and opposite. I again repeat that this state of alkalescence or acidity of the urine does not depend upon disease, and most probably depends solely upon the irritability of the stomach.

You will probably long since have asked,—What is the acid in the urine? And to this question I cannot yet give you a certain answer. I am not certain that the acidity is always produced by acid phosphate of soda, though at present this is the most probable answer that is known. It is probable that there is generally no free acid in the urine, but that the reaction arises from some acid salt; but I am certain that in some instances of strongly acid reaction, the quantity of phosphate of soda has been extremely small, much too small to produce the acidity, which was very decided.

The nature of the acid is not of great consequence, but the variations in the acidity of the urine in health give practical results of importance. For if you find that the urine passed at one hour of the day is highly acid, you may

find that the water made at another hour of the same day is nearly or quite alkaline; and if you are tempted to prescribe alkalies for the highly acid reaction of the urine made long after food, you may be led to prescribe acids for the alkaline reaction of the urine made soon after food. Thus according as you may see your patient early or late in the day, he may be put on a course of caustic potash or nitro-muriatic acid.

The reaction of test paper on urine made at any one hour of the day should never determine the use of acid or alkaline medicines. The different deposits which take place in the urine are far better tests of the state of the urine, and of the necessity for these remedies. If you are guided by the reaction of the test paper, the total quantity of urine made in twenty-four hours must be examined.

LECTURE VI.

ON URIC OR LITHIC ACID.

Gentlemen,—I shall occupy your attention to-day with the state in which the uric acid exists in the urine, with the variations in its amount in the healthy state, and with the causes of its precipitation.

In the twenty-seventh volume of the *Medico-Chirurgical Transactions* there is a paper on the state in which the uric acid exists in the urine. The conclusions at which I arrived were, that the uric acid is combined with ammonia, but that the urate of ammonia is modified in form and in solubility by the presence of common salt, and by other saline substances which exist with it. I was unable to prove the existence of two compounds of uric acid and ammonia; and although a paper in the *Annalen der Chemie*, vol. li., by Dr. Bensch, gives two compounds, yet I suspect the super-salt was a mixture of uric acid and urate of ammonia. This is a point of some importance, because Dr. Prout has stated that the acidity of the urine is chiefly caused by acid lithate of ammonia—a substance which, from my experiments, has no existence. I still think there is but one urate of ammonia, containing one equivalent of ammonia, and one equivalent of uric acid, and one equivalent of water, and the so-

called super-lithate of ammonia I consider to be a mixture of uric acid and urate of ammonia.

But this question does not require to be determined before an answer can be given to the question whether the acidity of the urine depends on any urate of ammonia. I have already shown you that the acidity of the urine is constantly changing. If, then, the acidity depends on uric acid or urate of ammonia, the variations of the acidity and of the uric acid must be directly proportionate the one to the other,—that is, when the urine is very acid there ought to be much acid urate of ammonia in it, and *vice versá*. Now experiment shows that no such relation exists. On the contrary, experiments demonstrate an inverse relation; when the urine is very acid, there is usually but little urate of ammonia present. When there is much urate of ammonia, not unfrequently the urine is nearly alkaline. Hence urate of ammonia is not the cause of the acidity of the urine, and the question whether there exists two urates of ammonia becomes of little interest for the physician.

The variations of the uric acid in the healthy state are of very great importance. The method which I have adopted for their determination is the following one:—

About two thousand grains of urine are accurately weighed, and poured into a glass like this. About a drachm of strong hydrochloric acid to an ounce of urine is then added; that is, about four drachms of strong hydrochloric acid to the two thousand grains, and the whole is left at rest for twenty-four hours at least. At first no immediate change ensues; in the course of a few minutes, as you see, a cloudiness forms, and gradually extends throughout the mixture in about three or four hours, (more or less, according to the strength of the acid, and the quantity of urate of ammonia present). This cloudiness changes into a sediment, which becomes more and more coarse and crystalline, and

falls to the bottom, or is deposited on the sides of the glass, thus. If the first cloudiness is examined, it is found that the whole is dissolved by heat, and by the microscope it is seen to be granular, and not crystalline. The coarse powder which afterwards forms is seen to be highly crystalline; frequently it consists of tufts of crystals insoluble by heat. I consider the first precipitate to be urate of ammonia, and not hydrated uric acid. The urate of ammonia is precipitated, because it is more insoluble in the dilute hydrochloric acid than in the urine, and therefore it falls, forming the cloud, which is soluble by heat. By long contact with dilute hydrochloric acid, the urate of ammonia is gradually changed into crystalline uric acid.

Urate of ammonia, in fact, is not a substance which is immediately decomposed by dilute hydrochloric acid. If but little hydrochloric acid is added to the urine, it must be left, not twenty-four, but forty-eight hours or longer before the uric acid will crystallize out. After this has taken place the clear liquid is cautiously poured off; the crystals detached from the sides of the glass with a fine feather, thrown on a weighed dry filter, slightly washed with distilled water, and dried perfectly in vacuo, and re-weighed. The weight of the filter being known by subtracting it, the amount of uric acid in the quantity of urine taken is determined, and a simple calculation immediately gives the quantity of uric acid in 1000 grains. The quantity of uric acid which is found in healthy urine is exceedingly variable. The quantity of uric acid daily excreted varies from six to ten grains, and it is probable that at no two hours of the day is the quantity perfectly similar. Two quantities of urine of the same specific gravity do not by any means necessarily contain the same amount of urate of ammonia. I have not yet been able fully to satisfy myself what causes the amount to vary; but that the quantity is influenced by food appears

from the following experiments so clear that there can be no further discussion on this point. The average of five days' breakfast being about nine, and dinner between six and seven, I found—

	Spec. grav.	Uric Acid. Per 1000 grs. of Urine.	Acidity. Per 1000 grs. of Urine.
Urine between 1 & 2 P.M. ...	1025.0 ...	0.52 gr. ...	13.07 measures.
" 6 & 7 " ...	1025.7 ...	0.12 " ...	26.12 "
" 10 & 11 " ...	1026.7 ...	0.62 " ...	13.81 "

The quantity of uric acid I found to vary before food from 0.048 grain per 1000 grains of urine, specific gravity 1026.3 to 0.17 grain per 1000 grains of urine, specific gravity 1023.1. After food it varied from 0.39 grain per 1000 grains of urine, specific gravity 1021.0 to 0.92 grain per 1000 grains of urine, specific gravity 1031.1.

If vegetable food only was taken, the same variations were observed. For three days vegetable food only was taken. The second day the following numbers were obtained: breakfast at nine, dinner at a quarter past six.

	Spec. grav.	Uric Acid. Per 1000 grs. of Urine.	Acidity. Per 1000 grs. of Urine.
Water passed from 10½ to 2½ ...	1021.65 ...	0.565 gr. ...	8.89 measures.
" 2½ to 6½ ...	1024.0 ...	0.049 " ...	26.36 "
" 6½ to 10½ ...	1026.2 ...	0.636 " ...	3.29 "
" 6½ A.M ...	1024.2 ...	0.665 " ...	19.52 "

For three days animal food only was taken. The second day, breakfast at nine, dinner at six: meat and eggs only were eaten.

	Spec. grav.	Uric Acid. Per 1000 grs. of Urine.	Acidity. Per 1000 grs. of Urine.
Water passed at 2½ P.M. ...	1022.7 ...	0.440 gr. .	7.82 measures.
" 6½ " ...	1024.8 ...	0.049 " ...	21.46 "
" 11½ " ...	1029.9 .	0.770 " ...	16.50 "

The highest quantity of uric acid found when vegetable food only was taken was the third day, at half-past ten A. M! when the urine, specific gravity 1025.6, contained 1.01 grain uric acid per 1000 grains of urine, the acidity being 12.67 measures per 1000 grains of urine.

The highest quantity of uric acid when animal food only was taken was the third day, at a quarter to eleven at night; the specific gravity was 1027.8, and contained uric acid 1.022 grains per 1000 grains of urine; acidity 18.48 measures per 1000 grains of urine.

I have tried to determine what the effect of exercise on the excretion of uric acid may be, but I have not succeeded in obtaining any satisfactory proof worth detailing here. The conclusion which I deduce from the above experiments is, that in a healthy state the amount of uric acid excreted is but little influenced by the kind of food; the effect is the same whether animal or vegetable food is taken.

	Per 1000 grs. of Urine.			Spec. grav.
After animal food, the highest amount is	... 1.022	gr. of	uric acid	1027.8
After vegetable food, the highest amount is	... 1.010	„	„	1025.6
The lowest amount, before animal food, is	... 0.049	„	„	1024.8
The lowest amount, before vegetable food, is	... 0.049	„	„	1024.0

The uric acid, then, exists in the urine in the state of urate of ammonia,* altered in form and solubility by the common salt present, and the solubility is most probably affected by all the other saline substances present in the urine. And secondly, the quantity of urate of ammonia varies remarkably; though it is but little affected by different kinds of food, yet a few hours after any food, the amount is increased, and long after all food, the amount of urate of ammonia is excessively diminished.

* Lehmann considers the uric acid exists as urate of soda, and not as urate of ammonia.—*Ed.* 1849.

It by no means follows that because much urate of ammonia exists in the urine, therefore it should form a precipitate, and become evident to the eye as a distinct cloudiness. If the urine is less acid at one time than it is at another, when least acid it will dissolve more urate of ammonia than when it is most so. A simple experiment proves this well.

On the 23rd of April, 1843, six ounces of urine passed at three o'clock in the afternoon, were poured into two vessels of the same size and form, equal quantities of urine in each. To one a couple of drops of ammonia were added, and it was marked *A*. To the other nothing was added, and it was marked *B*. Both reddened litmus paper decidedly; *B* more strongly than *A*.

April 24th, 8 o'clock A.M. ...	*A* quite clear...	*B* cloudy.
„ 1 o'clock P.M. ...	*A* quite clear...	*B* thick.
April 15th, ...	*A* quite clear	*B* sediment settled to the bottom of the glass.
April 26th, ...	*A* quite thick throughout	*B* Fluid above the sediment still more clear.

At this date both fluids still reddened litmus paper strongly.

The reverse of this experiment is quite as decisive. If equal quantities of the same urine are taken, and one is made slightly more acid than the other, by either vegetable or mineral acid, the more acid urine (as you see in this glass) will give a deposit, when the less acid urine in that glass will remain quite clear. The deposit will be uric acid or urate of ammonia, according to the quantity of acid added.

In a note to the first edition of his work, Dr. Prout says, "I wish to state that I have adopted this general view of the subject—viz. that the deposition of amorphous sediments is indicative of an excess of lithic acid in the urine—chiefly

from its simplicity and convenience. The deposition of amorphous sediments, for the most part, indicates an excess of lithic acid in the urine, but by no means universally so, for they appear to be sometimes deposited in consequence of a very slight excess of acid in the urine."—P. 117, second edition, 1825, omitted in later editions.

Acid phosphate of soda, so far as my experiments at present prove, does not cause the conversion of urate of ammonia into uric acid.

Two quantities of urine, specific gravity 1024.0, were taken; to one, crystals of acid phosphate of soda were added, and dissolved in the urine. Both gave a precipitate of urate of ammonia. In twenty-four hours I could find no uric-acid crystals in the quantity to which the acid phosphate of soda had been added.

A repetition of this experiment gave me the same result.

I conclude, that a slight excess or increase in the acidity of the urine will cause the precipitation of urate of ammonia, and the less acid the urine is, the less likely is the urate of ammonia to be precipitated. I have shown you how the acidity varies, how the urine becomes more acid, and more nearly alkaline, at different hours of the day, and thus the precipitation of the urate of ammonia must be greatly influenced. If the urine tends to alkalescence, an excess of urate of ammonia will be dissolved, and will show no appearance of a superabundance of urates. If the urine is highly acid, a precipitate may occur, even when no excess of urate of ammonia exists in it.

Direct experiment shows that the precipitation of urate of ammonia depends generally on the action of both these causes conjointly. If a very small quantity of urate of ammonia exists in the urine, it may be highly acid, without a marked precipitation taking place. If a very large quantity of urate of ammonia is present in the urine, it may give no precipitate if the urine be alkaline.

Precipitation usually occurs when there is a slight excess of urate of ammonia, and a slight excess of acidity also, aided always by a low temperature.

In twenty-eight days' experiments the urine was passed five times in the twenty-four hours, of the following average specific gravity and quantity. Breakfast, at half-past eight A.M.; dinner at six P.M.

Eight A.M.	Eleven A.M.	Three P.M.	Six P.M.	Eleven P.M.
$15\frac{3}{4}$ ozs.	$9\frac{1}{4}$ ozs.	$6\frac{1}{2}$ ozs.	$4\frac{1}{2}$ ozs.	$7\frac{1}{2}$ ozs.
Spec. grav.	Spec. grav.	Spec. grav.	Spec. grav.	Spec. grav.
1018.90	1015.90	1021.83	1024.14	1023.02
Thick	Thick	Thick	Thick	Thick
seven times.	seven times.	sixteen times.	eleven times.	ten times.

We know that most uric acid exists in the urine after food, and most acidity just before food; but the most frequent precipitation is not at the hour when the uric acid is greatest, nor when the acidity of the urine is most. At six P.M. the acidity is much greater than at eleven A.M.; and the urine, eleven times out of twenty-eight, gave a deposit at six, and only seven times out of twenty-eight at eleven A.M., when the quantity of uric acid was very much greater than at six. At three the urine gave a deposit sixteen times in twenty-eight observations, from the joint action of the two causes.

In all experiments on the precipitation of urate of ammonia care should be taken to note the number of hours, after the urine was passed, before the precipitation began to appear; it may be immediately on cooling, or in twenty-four or forty-eight hours, or longer, that a precipitate occurs. Usually the lowest temperature the night after the urine was passed causes the urate of ammonia to fall, if it will fall at all.

Two quantities of the same urine, the one exposed to the light, the other in the dark, I have seen deposit urate of ammonia at different times; and frequent agitation will always hasten the formation of a precipitate. The deposit of urate

of ammonia is therefore the result of the conjoint action of three causes—1, decrease of temperature; 2, increased proportion of urate of ammonia to the water; 3, increased acidity of the urine. Sometimes one cause, sometimes the other, is the most efficient; but they are all usually concerned in causing the deposit, which is soluble by heat.

In the *Philosophical Transactions*, part ii., 1849, I have given the following table, to prove that the mere appearance of a precipitate of urate of ammonia from the urine is no evidence of the quantity of urates that exist in the urine.

		Spec. grav.	Acidity per 1000 grs. urine.	Uric acid per 1000 grs. urine.	Appearance.
Water passed at	7.35 P.M.	1029.2	15.54 measures	0.29;	thickish from urates.
	10.5 „	1027.2	—0 „	0.33;	clear.
Another day,					
water at	7.55 P.M.	1029.6	21.47 „	0.31;	thickish from urates.
„	10.45 „	1029.8	—0 „	0.90;	no deposit.
Another day,					
water at	5.10 P.M.	1028.3	14.90 „	0.52;	thick from urates.
"	11 „	1031.1	2.85 „	0.87;	clear.

As regards the precipitation of uric-acid crystals, this depends only upon the acidity of the urine, and is quite independent of the quantity of the urate of ammonia excreted, though the quantity of crystals that fall must depend on the quantity present; but the quality of the precipitate is determined solely by an over acid state of urine, or what comes to the same thing, a deficiency of alkali there. Whenever uric-acid crystals are found in the urine, it may be taken as a proof that the urine is more acid than it ought to be. Uric acid crystals are, in fact, by far the most delicate and trustworthy test for an over acid state of the urine.

The urine is sometimes so acid, that even in the kidneys, in the ducts of the mammary processes, uric-acid crystals may be found. Most commonly, however, twenty-four hours must pass, after the water has been made, before the uric-

acid crystallizes out from over acid urine. Not unfrequently forty-eight hours are required. The time, to some extent, varies, not only with the temperature, but with the degree of acidity.

I have already stated that urine which contains a drachm of strong hydrochloric acid to the ounce of urine, at first gives a precipitate of urate of ammonia, which, in from three to four or more hours, according to the temperature, is decomposed. The whole of the uric acid cannot safely be considered to be crystallized out under twenty-four hours, even when the acidity is very great.

Of the causes which produce the numerous different forms in which uric acid occurs, I am not yet able to give you any satisfactory account. In healthy urine, the uric acid, when precipitated by another acid, generally gives lancet-shaped crystals, very frequently forming tufts, or microscopic calculi.

In judging of the nature of the deposit from the microscopic crystals in the urine some care is requisite. You will come to all kinds of false conclusions, unless, whenever it is possible, you collect a sufficient quantity of the sediment to examine it chemically.

I have a word or two here to say on the treatment of the two kinds of uric deposits. It divides itself into two distinct courses—the first depends on the presence of uric-acid crystals; the second, on their absence; that is, on the urate of ammonia occurring without any uric-acid crystals.

1. If uric-acid crystals are present, there are two things to be done—namely, to give alkalies and to forbid and remove all that may become acid. For the first, caustic alkalies, carbonated alkalies and earths; saline draughts, and phosphate of soda,—these must be regulated according to the peculiar symptoms of the patient; for the second, vegetable acids, sugar and starch in the food, should as far as possible be prohibited. The removal of acids by the skin, and car-

bonic acid by free respiration and exercise, is of still greater benefit; shortly the uric-acid crystals indicate that acidity must be removed, neutralized, and prevented.

2. When uric-acid crystals are absent, and urate of ammonia is deposited alone, it is a proof that no great excess of acid is present in the urine, otherwise uric-acid crystals would be formed; and though, by lessening the acidity of the urine, we can keep the urate of ammonia dissolved in it, and so hinder it from appearing, yet this is not the best mode of proceeding for effecting even this purpose. An extra glass of water, or soda-water, or some slight diuretic, as nitre, is more sure to keep the urate of ammonia in solution by increasing the quantity of urine, and this is the best palliative treatment; whilst the curative treatment consists in lessening the quantity of food taken, by smaller meals, and in lessening the acidity of the urine, by increased exercise. Thus you will best prevent deposits of urate of ammonia, and the alkalies may be kept until uric-acid crystals appear, when they are as necessary, as in urate-of-ammonia deposits they are unnecessary, for the treatment.

LECTURE VII.

ON OXALATE OF LIME AND SULPHATES.

Gentlemen,—Oxalate of lime is so frequently found in the urine of those who are in a good state of health, that I do not consider it as indicating any disease, but only a disorder of no serious importance. It scarcely indicates a more serious derangement of the general health than a deposit of urate of ammonia does. It may occasionally be found in the urine of all who lead sedentary lives, taking insufficient air and exercise, and more food than is requisite for the daily wants of the system. I have found it in the urine of those who are free from every complaint. Even in the urine of healthy children it may frequently be seen. I have met with it in every kind and stage of disease. In the fracture wards of St. George's Hospital I have very frequently found it. The most severe case I ever saw, was an artist, aged thirty, dying of abdominal aneurism. In cases of indigestion, especially where flatulence occurs; in cases where no indigestion ever was felt; in skin diseases; in cases where the skin never was affected; in cases of acute rheumatism, of acute gout, of fever; in sciatica in a gentleman, seventy-four years old, with spermatorhœa; and in the diseases of women and children, octahedral crystals occur. So frequently is oxalate of lime mixed with urate

of ammonia in sediments and calculi, that I have returned to the conclusion which Dr. Prout originally published in the second edition of his work. After giving some details of twelve cases of oxalic calculus, he says, " We are authorized to draw the following conclusions—6th. That from the dissection of calculi formerly mentioned, it appears that the oxalate-of-lime diathesis is preceded and followed by the lithic acid diathesis—a circumstance which seems to be peculiar to these two forms of deposit, and when taken in conjunction with the other circumstances already related, appears to show that they are of the same general nature, or, in other words, that the oxalic acid merely takes the place, as it were, of the lithic acid, and by combining with the lime naturally existing in the urine, forms the concretion in question. 7th. (Dr. Prout continues :) The diathesis being of a similar nature, the principles of treatment adapted for counteracting the original tendency to it must be also similar." (Page 159, Second Edition.) And, as a medicine, muriatic acid was used to change the diathesis from the oxalate of lime to the lithic acid.

I find that the two deposits together may be met with daily on careful examination, by the microscope, of the urine in different cases of disease ; and if the examination is made at different hours of the same day on a case in which oxalate of lime occurs, we shall not unfrequently find three hours after food, that instead of urate of ammonia being mixed with the oxalate of lime, there is phosphate of lime and oxalate, and no urate of ammonia. Thus at one hour we may find oxalate of lime alone ; at another, oxalate of lime with urate of ammonia ; and at a third examination, oxalate of lime and phosphate of lime, or phosphate of lime only ; the variations in the acidity of the urine being the chief cause of the differences in the deposit.

Oxalate of lime is so insoluble in distilled water, that it

might well be considered to be insoluble in the urine. Its occurrence in the form of crystals shows, however, that it cannot be insoluble; for crystalline form implies deposit from solution. Careful observation of the urine also shows that the oxalate of lime is soluble therein.

A medical man, in very tolerable health, aged fifty-nine, passed about a drachm of water at half-past ten in the morning, saying that he had a good deal of irritation. I examined it immediately. It was acid, and had a slight cloudiness. Under the microscope I saw many globules, like mucus, some of them slightly serrated. I saw some dead perfectly-formed spermatozoa, and a great many particles looking like the bodies of spermatozoa, tail-less—that is, slightly triangular and highly refracting. I looked most carefully for oxalate of lime, expecting to find it; but I could not find a single crystal anywhere, and no urate of ammonia was to be seen. At nine the following morning I again looked at the drachm of urine. There was a very transparent cloudy sediment, like mucus, only more transparent, occupying nearly one-fourth of the liquid in height. On examining a drop by the microscope, myriads of small crystals of oxalate of lime were seen, and not a particle of urate of ammonia or uric acid.

I had for some time passed over similar observations, by supposing that my first examination had not been made with sufficient care, but from the above, and other cases that have since occurred to me, I am certain that it sometimes requires many hours for the oxalate of lime to crystallize out. You cannot say that no oxalate of lime exists in any urine until at least twenty-four hours have elapsed from the time of the passing of the water.

It requires no skill and no preparation of the urine to find the oxalate of lime. The urine should be left to stand for twenty-four hours in a bottle, or tall glass; the upper

part of the fluid should be poured off, and the last few drops remaining in the glass or bottle should be examined. A magnifying power of 320 times is generally sufficient, but the crystals are sometimes so small, that twice this power is necessary to determine the form. Generally oxalate-of-lime octahedra are thus found without the least difficulty, sometimes in large single crystals, very frequently in aggregations of small octahedra, forming microscopic calculi. Dr. Golding Bird was the first observer who stated that these crystals, which had for some time previously been observed in urine, were oxalate of lime. The chemical proof is difficult, if not impossible, to obtain, for the octahedral crystals are rarely present in sufficient quantity to admit of perfect examination.

In the twenty-seventh volume of the *Medico-Chirurgical Transactions* I mentioned the following case, which, for its duration and intensity, is worthy of your notice:—

John S——, aged forty-seven, formerly a soldier, was admitted an out-patient of St. George's Hospital, July, 1842. In March, 1843, I examined the deposit which had been continually observed in the urine. It consisted of innumerable crystals of uric acid, mixed with octahedral crystals. For twenty years he had suffered more or less from gravel. In 1828 he had rheumatic fever, and says he was confined to bed for eleven weeks. The small joints of the fingers are now larger and stiffer than natural, and occasionally very painful. The urinary sediment was thrown on a filter, and washed with distilled water; the red residue was then reduced to a fine powder, and treated with dilute boiling hydrochloric acid, which left most of the uric acid undissolved. The acid liquid was filtered, and ammonia gave a very considerable precipitate when added in excess. When evaporated to dryness and heated on platinum, the muriate of ammonia was driven off, and the residue effervesced

strongly; when heated much longer, the ash was alkaline, and with difficulty soluble in water. It was soluble in acids, and, when neutralized, gave a precipitate with oxalate of ammonia.

In January, 1849, I again examined the water of this patient, and found precisely the same appearance, which he says has continued more or less ever since I first saw him.

The occurrence of calculi of oxalate of lime simultaneously with octahedral crystals in the urine also confirms their composition.

On the 4th of October, 1843, I examined the urine of a patient of Mr. Cutler's, and at the same time three calculi passed—one in July, the second in August, and the third in September. Late in October a fourth calculus was passed, which I also examined. The urine under the microscope contained multitudes of octahedra, mixed with some crystals of uric acid. All the four calculi consisted of oxalate of lime and uric acid. Further proof of the nature of the octahedral crystals is hardly necessary. But I have lately found that if oxalate of lime is dissolved by heat in dilute hydrochloric acid and set aside, after the lapse of many days octahedral crystals will very frequently be formed. The less oxalate of lime present, and the more acid the solution, the slower the crystals form. Rhombs and rhombic plates occur in consequence of the variations of circumstances, which I have not yet fully investigated; but with care you will often succeed in making oxalate of lime crystallize in octahedra. In no experiment have I formed the crystals which Dr. G. Bird has called dumb-bell crystals of oxalate of lime. I cannot as yet form these artificially; they do not frequently occur in the urine, and their appearance does not seem to me to indicate any point of importance. (They are insoluble in distilled water, and this may prevent mistakes.)

Perhaps oxalate of lime occurs in the urine in a third form,

which has been observed also by M. Donné ; and as the appearance may lead to a wrong diagnosis, it requires to be mentioned here. I sometimes find, with or without octahedral crystals, little flattened discs, the size of very small blood-globules ; when rolling over, they may very easily be mistaken for blood-globules. They vary much in size, some being much smaller than any blood-globule. I have seen these discs mixed with octahedra, and dumb-bell-like crystals ; in fact, the smallest dumb-bell crystals form minute flattened discs. They are not soluble in distilled water. I at first considered they were blood-globules ; they occur in very small quantity, and require further examination.

I am not yet satisfied whether most oxalate of lime occurs in the urine before or after food ; but I have seen a visible sediment of oxalate of lime in urine passed after dinner, when the microscope has detected but little in that passed before dinner. In another case I found very little oxalate of lime in the water made late at night, but large quantities in that made early the following morning. Further experiments are requisite on this subject.

There is nothing peculiar in the appearance of the urine which gives a deposit of oxalate of lime. The appearance of the urine is no guide to the deposit. If oxalate of lime occurs alone in the urine, it is generally paler than when urate of ammonia occurs also. The urine frequently is of high specific gravity, and quickly deposits nitrate of urea, when nitric acid is added ; but excess of urea is not always characteristic of urine containing oxalate of lime.

The presence of oxalate of lime may be suspected when sudden changes in the quantity of urine made in twenty-four hours are observed. Patients in tolerable health, who for days or weeks have been passing urine which became cloudy on standing, occasionally observe a sudden change. The water, without any known reason, will become clear,

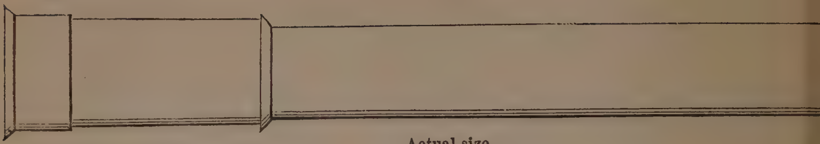
and sometimes very plentiful, and generally an increase in the urgency of making it occurs.

As an example, I may mention the case of a gentleman who was passing about twenty-nine ounces of urine, specific gravity 1023.8, in twenty-four hours. It gave a thick deposit of urate of ammonia. The following day, without any change in diet, or quantity of fluid drunk, or medicine of any kind, fifty-four ounces were passed, specific gravity 1018.1 ; and on examination myriads of octahedral crystals were seen.

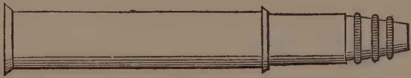
I have known the sudden increase of water and change in the deposit attributed to dilute nitric acid and to alkalies ; but, as in the above case, it may take place independently of medicine.

Sometimes there is a slight degree of turbidity in urine containing oxalate of lime, from the immense number of octahedral crystals in it. These, on standing, form a distinct white, cloudy deposit. The cloudiness more commonly arises from an excess of mucus and epithelium. Neither the mucus, the epithelium, nor the oxalate of lime, dissolve when heat is applied ; so that heat gives a ready means of distinguishing these substances from urate of ammonia. Oxalate of lime is soluble by heat, on the addition of hydrochloric acid ; but the only means of determining the presence of small quantities of oxalate of lime, or of mucus, or epithelium, is the microscope. For this purpose a small microscope, made by Powell,* at a comparatively small expense, and yet defining as perfectly as the most expensive instrument, is all that is required for this or for any other question regarding the microscopical appearances of the urine.

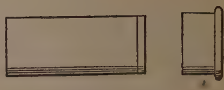
* Powell and Leland, opticians, 4, Seymour Place, Euston Square.



Actual size.



Microscope.



Object cage.

You will be inclined to ask—Is the detection of oxalate of lime a matter of importance? The true answer is, that in the great majority of cases it is not of more importance than the deposit of urate of ammonia.* It may, however, form a calculus, and it always indicates some disorder of the digestion.

One word as to treatment. In cases of indigestion with oxalate of lime, ammonia, mineral acids, quinine, and iron, are indicated. Animal diet, cold bath, and brandy-and-water, appear frequently to give much benefit. When there is irritable bladder, with excess of mucus and epithelium, camphor and compound tincture of camphor have proved very useful. If blood-globules occur with the octahedral crystals, the presence of gravel or calculus in the bladder or kidneys becomes probable.

Whether sugar gives rise to oxalic acid in the urine is far from being determined. When no sugar at all has been taken, and the patient has lived on animal food alone, I have found oxalate of lime in plenty in the urine. I have already stated to you that sugar is most probably converted into some acid in the stomach; and that sugar should therefore be avoided, in some cases of indigestion, is most certain.

That the use of distilled water, instead of spring water

* Lehmann, in the last edition of his Animal Chemistry, has come to the same conclusion.

will lessen the quantity of lime passing through the body is also most certain. By this means the oxalate of lime may be somewhat lessened, but by air, exercise, and careful diet, the disorders which accompany it may be cured.

On the Sulphuric Diathesis, or Excess of Sulphates in the Urine.

No new term should be introduced into medicine unless the necessity for it is urgent: we have too many words already. I have placed this here, because at the very least it will give a degree of distinctness to your knowledge of the phosphatic diathesis which could not otherwise be obtained. I have never seen a deposit of sulphates in the urine. There may be an excess of sulphate in the urine, but it never shows itself. It may be made to appear; that is, an insoluble salt of sulphuric acid may be formed, when we please, in the urine after it is passed. But this does not take place in the body. Whether the urine is very acid, or very alkaline, or ammoniacal, makes no difference so far as the solution of the sulphates is concerned. Sulphate of soda or potash never forms a precipitate. Thus, then, we may have a great excess of sulphate in the urine, when, so far as the eye can judge, there might be a deficiency; by causing some insoluble salt, as the sulphate of baryta, to form, we may decide whether much or little sulphuric acid is present; but unless we thus precipitate all the sulphuric acid, we can come to no conclusion whatever regarding an excess or deficiency of that substance in the urine. There is no state of the urine, whether of acidity or alkalescence, or any other state whatever, in which the sulphates, even though present in great excess, will of themselves form a precipitate. How great soever the quantity of sulphate present may be, it will never make itself apparent. Yet when

we please, there is no substance more easily made apparent and collected and weighed than the sulphuric acid. Here, then, it is evident that mere inspection of the urine will not say whether much or little sulphate is present, nor, indeed, whether any at all is there.

The method to be followed is this:—About five hundred grains of urine are weighed, and chloride of barium is then added in excess, and a few drops of hydrochloric or nitric acid are employed to ensure the solution of the phosphate of baryta. Heat is then applied, and the liquid is boiled for a few minutes briskly. The sulphate of baryta is then filtered, and well washed, until the clear liquid is perfectly free from chloride of barium. The filter is burnt, and the residue weighed; thus the amount of sulphate of baryta in a known quantity of urine is determined.

The average of five days, breakfast being about nine, and dinner between six and seven, I found to be as follows:—

	Specific Gravity.		Sulphate of Baryta, per 1000 grs. of urine.
Urine secreted between 1 and 3 P.M.,	1026.1	... ...	7.77 grs.
" " 3 and 6 "	1026.5	... ...	7.93 "
" " 6 and 11 "	1031.1	... ...	11.85 "

The quantity of sulphuric acid I found to vary, before food, from 8.56 grs. sulphate of baryta, per 1.000 grs. of urine, specific gravity 1027.6, to 7.07 grs. per 1000 grs. of urine, specific gravity, 1025.3. After food, it varied from 15.23 grs. sulphate of baryta, per 1000 grs. of urine specific gravity 1033.9, to 9.49 grs. per 1000 grs. of urine, specific gravity 1029.3.

If vegetable food only was taken, as bread, with a little rice and water, an increase was also observed after food. On the third day of such food,

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
At 3 P.M.	1027.56	... ... 9.53 grs.
" 6 "	1028.58	... ... 9.46 "
" 11 "	1031.86	... ... 13.68 "

When animal food only was taken, for three days consecutively, the third day,

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
At 3 P.M.	1023.92	... ... 8.36 grs.
" 6 "	1025.44	... ... 9.30 "
" 11 "	1026.24	... ... 11.14 "

Comparing these numbers with the average, I consider that no distinction can be drawn between the influence of animal or vegetable food on the amount of sulphates; after both, the sulphates in the urine are increased.

By taking no food in the morning, and by taking strong exercise before dinner, I attempted to determine the effect of exercise. Dinner at a quarter-past six,

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
Urine passed at 4 P.M.	1029.52	... ... 8.76 grs.
" " 6¼ "	1031.18	... ... { 11.26 " / 11.23 "
" " 10¼ "	1029.04	... ... 12.34 "

Food has more influence on the sulphates than exercise has; but from the above and other experiments, I believe that exercise does cause an increase in the amount of sulphuric acid in the urine.

The effect of different medicines is more clearly made out. From thirteen to twenty drops of strong sulphuric acid were taken in water on five different days, but I could not detect a decided increase in the sulphates in the urine. A course of dilute sulphuric acid was then tried — three

drachms the first day, and two drachms the three following days: still I found no manifest increase of sulphates. For three more days the course was continued. Seventeen drachms of dilute sulphuric acid having been taken in the eight previous days, I found the following numbers:—

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
1 P.M.	1026.0	7.37 grs.
3 "	1025.9	7.22 "
6½ "	1023.3	5.43 "
10 "	1028.1	10.66 "

Hence there was no proof of the sulphuric acid passing off in the urine. Therefore the amount of sulphates in the whole quantity of water passed in twenty-four hours, for three successive days, when no sulphuric acid had been taken, was compared with the amount of sulphates in the whole quantity of water passed in twenty-four hours for the three succeeding days, when sulphuric acid had been taken. The food and exercise were the same for the six days.

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
1st day, 37½ ozs.	1024.2	7.75 grs. 7.66 "
2nd " 42 "	1023.4	9.18 " 9.20 "
3rd " 34 "	1026.1	7.83 "

During each of the three following days half an ounce of dilute sulphuric acid was taken in distilled water.

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
4th day, 46 ozs.	1024.2	9.56 grs. 9.64 "
5th " 42 "	1024.0	11.66 " 11.64 "
6th " 43 "	1025.4	13.10 " 12.81 "

Hence dilute sulphuric acid, when taken in very large doses, does cause an increase in the amount of the sulphates in the urine.

For five days, sixty-one grains and a-half of dry sulphur were taken. The average was—

	Specific Gravity.	Sulphate of Baryta, per 1000 grs. of urine.
Before dinner	1022.5	8.69 grs.
After dinner	1027.1	14.01 „

Hence the sulphates are above the average when sulphur is taken.

123½ grains of dry sulphate of potash, taken at eleven A.M., increased the sulphates; so that at half-past ten P.M., the urine, specific gravity, 1032.4, contained 20.49 grains sulphate of baryta, per 1000 grains of urine. After two drachms of sulphate of magnesia I have seen urine, specific gravity 1024.3, contain sulphate of baryta 22.55 grains.

The general conclusions I have come to are these—

1. That the sulphates in the urine are much increased by food, whether it be vegetable or animal.

2. Exercise does not produce so marked an increase in the sulphates.

3. Sulphuric acid, when taken in large doses, increases the sulphates in the urine; in small quantity, it produces little or no effect.

4. Sulphur, when taken, increases the sulphates in the urine, and sulphate of soda or magnesia produces the greatest effect on the quantity of sulphates in the urine. (See *Philosophical Transactions*, 1849.)

Knowing the variations of the sulphates in health, and knowing the effect of remedies, we can judge of the influence of diseases on the sulphates in the urine.

In a future course, I hope to be able to bring before you the variations of the sulphates in disease, to prove to you on

what their increase or diminution in disease depends. If I can show you that an excess of sulphates is peculiar to any one class of diseases, you may perhaps be able, in a doubtful case, from the amount of sulphates in the urine, to determine whether the case belongs to this class or not.

What I want now is to show you, that as the eye alone cannot tell you if much or little sulphate is present in the urine,—because the sulphate will never show itself, although it can most readily be made to appear,—therefore, that by merely looking at the urine—that is, without examining it chemically—you can learn nothing regarding the sulphuric diathesis. In my next lecture I will show you that the same conclusion is the truth regarding the so-called phosphatic diathesis.

LECTURE VIII.

ON THE ALKALINE AND EARTHY PHOSPHATES.

Gentlemen,—Previous to entering on this subject it is most necessary that I should shortly call to your remembrance the peculiarities of the salts of phosphoric acid. In consequence of some statements in medical works on animal chemistry, from which I differ, it is necessary, in order to make this subject clear to you, to commence with the properties of phosphoric acid, according to the views of Professor Graham.

The compound of oxygen and phosphorus which has received the name of phosphoric acid, is capable of combining, not with one equivalent of oxide of potassium, oxide of sodium, or oxide of a metal, but with three equivalents. Phosphoric acid is therefore called a tribasic acid. The three bases need not be all of one substance, all oxide of potassium, or oxide of sodium, or all oxide of calcium. They may all be different, or there may be two bases of one kind and one of another. From the study of the salts of phosphoric acid, Professor Graham was led to the important discovery, that oxide of hydrogen, or water, like oxide of sodium or calcium, might combine with phosphoric acid, or form one of the bases, whilst soda or potash formed the other two. Thus one equivalent of phosphoric acid, being tribasic, might combine

with an equivalent of water for one base, and two equivalents of soda for the other two; or it might combine with two equivalents of water and one of soda, or it might form a definite compound with three equivalents of soda. Thus we have various salts of phosphoric acid, in which one equivalent of phosphoric acid is combined with three equivalents of any base, or with two equivalents of one base and one of another, or with three equivalents of different bases, and water, or the oxide of hydrogen may be a base. For example: we have a compound in which one equivalent of phosphoric acid is combined with two equivalents of water and one equivalent of soda. This substance is called acid phosphate of soda; it has an acid reaction, and it is a tribasic salt: or we may have a compound in which one equivalent of phosphoric acid is combined with one equivalent of water and two equivalents of soda. This is the common phosphate of soda. It has an alkaline reaction, and it also is a tribasic salt. Lastly, we may have a compound in which one equivalent of phosphoric acid is combined with three equivalents of soda. This is alkaline phosphate of soda. It has a stronger alkaline reaction than the previous salt, but it is not more tribasic. There are three bases in each of the three salts; whether there be three equivalents of soda, or two of soda and one of water, or one of soda and two of water, makes no difference, so far as the tribasic properties of the phosphoric acid is concerned. All these three salts when in solution give a yellow precipitate with nitrate of silver.

If the common phosphate of soda is heated to expel an equivalent of water, we then have, instead of two equivalents of soda and one of water, combined with phosphoric acid, two equivalents of soda only and no water. The effect of the heat has been, to change the properties of the phosphoric acid, so that instead of combining with three bases,

it can now only combine with two; it has become bibasic. The two bases may be both soda, or one soda and one water, or one soda and one potash. The solutions of these bibasic salts no longer give a yellow precipitate with nitrate of silver but a white one. Such phosphoric acid used to be called pyrophosphoric acid; now bibasic phosphoric acid.

Finally, if the acid phosphate of soda—that is, tribasic phosphoric acid, combined with one of soda and two of water, is heated until two equivalents of water are driven off, then what was called metaphosphate of soda remains. This phosphoric acid can combine with one equivalent only of any base, hence it is monobasic phosphoric acid. Monobasic and bibasic phosphoric acids result from the effects of heat upon the tribasic phosphoric acid. So far as I know, they never occur in the human body; the temperature is not sufficient to produce them We are only concerned with the different compounds of the tribasic phosphoric acid, and there is no difficulty if you remember that oxide of hydrogen or water is capable of combining with phosphoric acid.

The tribasic compounds which I have chiefly to mention are these—

Common phosphate of soda	$= PO_5\ 2NaO\ \ HO +$	water of crystallization.
Acid phosphate of soda	$= PO_5\ \ NaO\ 2HO +$	"
Phosphate of ammonia & magnesia	$= PO_5\ 2MgO\ \ NH_4O +$	"
Gelatinous phosphate of lime ...	$= PO_5\ 3CaO$	
Crystalline phosphate of lime ...	$= PO_5\ 2CaO\ \ HO +$	"

It is probable that all the three phosphates of soda occur in the blood; if not at the same time, yet at different times and under different circumstances. The common phosphate and the alkaline phosphate certainly exist in the blood; in the urine the acid phosphate and more rarely the common phosphate occur. There are earthy phosphates also present

in the urine—namely, phosphate of lime and phosphate of magnesia, but the phosphates of soda are almost without exception always present, in much greater quantity than the earthy phosphates. The phosphates of soda are very soluble in water, and in acid or alkaline urine they are held in solution. They never fall as a precipitate, but, like the sulphates of potash or soda, they may be in excess, and yet not make themselves apparent ; we can make them appear, but they also do not show themselves under any circumstances. The lime and magnesia phosphates, on the contrary, are not very soluble in water, and they are nearly insoluble in alkalies, but they are very soluble in acids of any kind, even in acid phosphate of soda. Usually when the earthy phosphates are precipitated from the urine, there remains in solution three or four times as much phosphate of soda. The phosphates of soda in the urine are the most abundant. The earthy phosphates as regards quantity are of much less importance Hence, as to quantity, if we speak of phosphates in the urine, we ought to limit that term to the alkaline phosphates, and not apply it to the earthy alone, as is at present done. The amount of earthy phosphates precipitated when the urine becomes alkaline is solely dependent on the amount of the earths present—that is, on the quantity of lime and magnesia passing out of the system. By taking lime-water or magnesia, or by adding these to the urine, we may increase the earthy phosphates ; and finally, if we add enough, we may precipitate all the phosphoric acid in combination with these earths, and leave no phosphate of soda in solution. On the contrary, if we could take away all the lime and magnesia, though we had a great excess of phosphate of soda in solution, yet we should have no precipitation on the addition of alkalies ; but on adding any lime or magnesia to such alkaline urine, an immediate precipitation of phosphate of lime or phosphate of magnesia would occur.

From this it is evident that, as regards the amount of phosphoric acid in the urine, it is necessary to know the amount of alkaline as well as of earthy phosphates. Both together will give us the variations of the phosphoric acid, while the earthy phosphates, taken alone, indicate only the quantity of earthy matter in the urine—the amount of lime and magnesia, and nothing more.

The method to be followed in determining the variations of the phosphates is this :—About 1000 grains of urine are weighed, and the earthy phosphates precipitated by pure ammonia free from carbonate. They are filtered, washed with ammoniacal water, heated to redness, adding at last a drop or two of nitric acid; thus the earthy phosphates are determined by weighing the residue. The alkaline phosphates are estimated by taking about 500 grains of urine, adding an excess of chloride of calcium and then pure ammonia. Thus all the phosphoric acid is precipitated as phosphate of lime. This is filtered, well washed, and the filter and precipitate burnt with a drop or two of nitric acid. If the filtration has been slow, it is necessary to re-dissolve the residue in the platinum crucible by hydrochloric acid, and to re-precipitate by pure ammonia, after which the filtration will take place very rapidly. After being burnt, the crucible is weighed, and by deducting the previously determined earthy phosphates the difference may be taken as alkaline phosphate. (See *Philosophical Transactions* for 1845, p. 365.)

The average of five days; breakfast about nine, dinner at six :—

	Specific Gravity.		Earthy Phosphates, per 1000 grs. urine.		Alkaline Phosphates.
Urine between 6 & 7 P.M ...	1027.9	...	0.49 gr.	...	7.56 grs.
„ 12 & 1 at night.	1030.0	...	1.45 „	..	5.77 „

The earthy phosphates I found to vary,

		Per 1000 grs. urine.		Spec. grav.
Before food	.. ...	from 0.21 gr.	...	1028.2
		to 0.75 „	...	1028.0
After food	... ...	from 0.97 „	...	1027.3
		to 1.91 „	...	1033.2

The alkaline phosphates vary,

		Per 1000 grs. urine.		Spec. grav.
Before food	... ...	from 6.50 gr.	...	1022.8
		to 8.10 „	...	1028.0
After food	.. ...	from 4.72 „	...	1033.2
		to 6.67 „	...	1025.5

If vegetable food only was taken—bread, water, tea, and wine—for three days,

	Specific Gravity.		Earthy Phosphates, per 1000 grs. urine.		Alkaline Phosphate.
The third day, at 6 P.M.	1027.7	...	0.37 gr.	...	8.19 grs.
„ 11 „	1032.1	...	1.86 „	...	5.56 „

When animal food only was taken, for three days consecutively,

	Specific Gravity.		Earthy Phosphates, per 1000 grs. urine.		Alkaline Phosphate.
The third day, at 6 P.M.	1024.3	...	0.42 gr.	...	4.04 grs.
„ 11 „	1024.8	...	0.81 „	...	4.31 „

From the comparison of these numbers with the average previously stated, it appears that the earthy phosphates are rather less, when animal food is taken, than when vegetable food is taken ; that they are in excess after either diet ; and I found that even when animal food and distilled water only were taken, there was a decided increase afterwards, though much below the average of ordinary diet. The alkaline phosphates are in excess, when bread alone is taken ; when meat alone was taken, the quantity of alkaline phosphate present in the urine was considerably below the average on mixed diet.

By taking no food in the morning, and by taking strong exercise before dinner, I tried to determine the effect of exercise on the amount of phosphoric acid in the urine. I came to the conclusion that the amount of earthy phosphate was not influenced by exercise, but that the amount of alkaline phosphate is increased by exercise, but not to the same extent as by bread.

The influence of different medicines on the amount of earthy phosphates is very easily shown. Chloride of calcium, thirty-five grains, taken at a quarter to one o'clock, continued perceptible for ten hours. Dinner at half-past five,

	Specific Gravity.	Earthy Phosphate, per 1000 grs. urine.
At 3 P.M. ...	1023.8	1.26 gr.
„ 5½ „ ...	1022.3	1.08 „
„ 10¼ „ ...	1030.1	1.02 „

If no chloride of calcium was taken,

At 3 P.M. ...	1027.4	0.60 gr.
„ 5½ „ ...	1027.0	0.36 „
„ 10¼ „ ...	1032.7	0.97 „

Sulphate of magnesia, forty grains, perfectly dry, taken at a quarter to one,

	Specific Gravity.	Earthy Phosphate, per 1000 grs. urine.
At 5½ P.M.	1029.3	0.90 gr.
„ 9½ „	1032.3	1.64 „

A patient of Dr. Seymour's, in St. George's Hospital, took senna, with two drachms of sulphate of magnesia, in the morning. The medicine did not act on the bowels. At three P.M., specific gravity 1027.6; earthy phosphate, 2.99 grs. per 1000 grs. urine.

Another patient, under the same circumstances; 1026.2 specific gravity; earthy phosphate, 2.93 grs. per 1000 grs. urine.

Calcined magnesia was then tried: forty-five grains produced no increase in the earthy phosphates in two hours and three-quarters; in five hours there was a marked increase; in seven hours and a half there was a still further increase, which was very marked at the end of twelve hours; and from the large quantity of earthy phosphate which was present the following morning, perhaps the magnesia had not ceased to act in twenty-four hours.

30.8 grains of dry sulphate of magnesia produced a decided increase in the earthy phosphates in five hours.

In a child twenty months old, 15.4 grains of magnesia, taken about half-past seven A.M.,

	Specific Gravity.	Earthy Phosphate, per 1000 grs. urine.
At 11 A.M. to 1 P.M. ...	1025.3	0.62 gr.
„ 3 P.M. to 5 „ ...	1027.7	1.57 „

The same child, after 19.3 grs. of magnesia, at eight A.M.,

	Specific Gravity.	Earthy Phosphate, per 1000 grs. urine.
11 A.M. to 1 P.M. ...	1014.4	0.45 gr.
3 P.M. to 5 „ ...	1017.4	0.80 „

When no magnesia was taken,

3 to 6 P.M. ...	1018.5	0.36 „

The general conclusions I came to are these:—

1. That the amount of earthy phosphates depends chiefly on the amount of earthy matter taken into the body.

2. That the amount of alkaline phosphates depends chiefly on the food, but that exercise also causes some increase of these phosphates. These, being soluble in fixed alkalies and in carbonate of ammonia, never fall as a precipitate when the urine becomes alkaline, although the earthy phosphates then become apparent. These last indicate only the quantity of earthy matter in the urine.

In the first edition of Dr. Prout's work, he called the

phosphatic diathesis the phosphatic or earthy diathesis. He corrected the earthy for the term alkaline diathesis; and then, in the second edition, he returned to the phosphatic, or earthy diathesis. From my experiments I consider that what Dr. Prout, and all others after him, call the phosphatic diathesis, is, in fact, nothing else but the precipitation of the earthy phosphates, in consequence of the alkalescence of the urine, and I consider that the term alkaline urine is the most fit to be applied thereto.

Urine which is alkaline from carbonate of ammonia, carbonate of soda, carbonate of potash, or phosphate of soda, is not able to dissolve the earthy phosphates, and they therefore, generally, fall as a precipitate. This precipitation of the earthy phosphates solely depends on the urine being or becoming alkaline. It will add not a little to the clearness of our view of urinary diseases, if this meaning of the term phosphatic diathesis is entirely given up, and its place supplied by the term alkaline urine; and it will give still more precision, if, when the earthy phosphates are precipitated by carbonate of ammonia, such urine be called ammoniacal urine; and if, when the earthy phosphates are precipitated by fixed alkali, this urine be called alkaline, from fixed alkali. The term phosphoric diathesis, I consider, ought to be used (as the term sulphuric diathesis is) to denote an increase in the total amount of phosphates, alkaline as well as earthy; and this term ought not to be applied to the mere precipitation (it might be of only a very small amount) of the earthy phosphate alone.

The total amount of alkaline and earthy phosphates in the urine never makes itself evident to the eye, for the phosphates of soda never fall as a precipitate. Like the sulphate of potash or soda we never can tell whether there is much or little present in any urine by merely looking at it. The earthy phosphates show exactly how much earthy matter—

that is, lime and magnesia—is present, but they are no index whatever to the amount of alkaline phosphates in the urine. I consider then, for these reasons, that what is now called the phosphatic diathesis ought to be called alkaline urine, and the term phosphoric diathesis ought to be applied to an increase in the total amount of alkaline and earthy phosphates; and if limited to one phosphate it ought to be given to the alkaline phosphate, because there is four or five times less of the earthy than of the alkaline phosphate in the urine; moreover, the term earthy diathesis, if used at all, ought to be given to urine which contains an excess of lime and magnesia, and not to the precipitation (it may be, of only a small quantity) of these substances in consequence of the urine having lost the property of dissolving them by becoming alkaline.

Alkalescence of the urine and an increase in the total amount of phosphates have no relation of any sort or kind to one another. They are totally distinct; in fact, they are rather opposite states. At least, ammoniacal urine generally contains only a small quantity of alkaline and earthy phosphates; and urine, which contains a great excess of alkaline and earthy phosphates, is generally highly acid, and remains so above the average time without undergoing decomposition.

As in the sulphuric diathesis, so in the true phosphoric, some method must be employed to cause the precipitation of all the phosphates. The addition of chloride of calcium and ammonia, free from carbonate, is generally sufficiently accurate for the purposes of comparison. No judgment by the eye of the quantity of the precipitate can with safety be relied upon; for quantities we must trust only to the balance. Where it is a question of quality, as of the presence or absence of albumen or sugar, the eye may be trusted; but for quantities, the eye alone will lead to great mistakes.

After a long inquiry into the total amount of phosphates

in various diseases, the conclusions at which I have arrived are these. The details of the cases which led to these conclusions are given in "The Lancet" for 1847.

1. The variations of the earthy phosphates are so dependent on the earthy matter (lime and magnesia) present in the urine, that no deduction from them as to the nature or state of the disease is possible.

2. Neither the earthy phosphates nor the alkaline phosphates are permanently increased in spinal diseases.

3. In fevers and acute inflammations of fibrous muscular or cartilaginous tissues, the total amount of earthy and alkaline phosphates is not increased.

4. In chronic diseases in which the nervous tissue is not affected, no deduction can be drawn.

5. Chronic cases of mania, melancholia, and general paralysis of the insane, gave no marked results.

6. In chronic diseases of the brain, and in chronic and even acute disease of the membranes, there is no increase in the total amount of earthy and alkaline phosphates.

7. In fractures of the skull, when any inflammation of the brain comes on, there is an increase of the total amount of phosphates. When there are no head symptoms, no increase of the phosphates is observed, even when other acute inflammations supervene.

8. In acute inflammation of the brain there is an excessive amount of phosphates in the urine. When the inflammation becomes chronic, no excess of phosphates can be shown to exist in the urine by the method of analysis that was employed.

9. In some functional diseases of the brain an excessive amount of phosphates is observable: this ceases with the delirium. Delirium tremens shows a remarkable deficiency in the amount of phosphates excreted, provided no food is taken. When food can be taken the diminution is not apparent.

As regards the treatment of cases in which an excess of alkaline and earthy phosphates exists in the urine, very little is to be said. The excess of phosphates is the result of the disease, and not its cause. Stop the disease, and you lessen the phosphates: it by no means follows that you would produce the smallest effect on the disease, if you could lessen the excretion of the phosphates. The excess of phosphates in the urine may lead you to a right diagnosis, but to consider that the increase of the phosphates in the urine constitutes the disease will only mislead you from the treatment of the real complaint.

As regards the treatment of those cases in which the earthy phosphates are precipitated alone from the urine, it resolves itself simply into the means of keeping the urine acid—that is, of hindering the urine from becoming alkaline, whether from fixed alkali or from carbonate of ammonia; but this belongs to my next lecture, on "Alkalescence of the Urine."

LECTURE IX.

ON ALKALESCENCE OF THE URINE, FROM FIXED AND VOLATILE ALKALI.

Healthy urine, when left to stand, becomes alkaline, but the time that must elapse before this occurs is very variable, depending not only on the temperature of the air, but on many other causes. Other things being the same; the higher the temperature the sooner the urine will undergo decomposition. Among other causes, it has been stated that the urine possesses vital properties, and therefore, on the loss of these, alkalescence occurs. I can tell you nothing about these vital properties, nor how they can hinder the change in the urine. I know that a great cause of this change is the presence of mucus, which is secreted by the mucous membrane of the bladder, and this may be considered as the ferment or cause of change in the urine. More or less of this substance may be always found to be present. To show its influence, I took equal quantities of the same urine; one portion (A) I filtered through very fine filtering-paper; the other portion (B) remained as it was passed. Both quantities were exposed to similar influences, as regarded temperature, position, &c. Both remained acid until the eleventh day, when A (*filtered*) was acid, but B (*unfiltered*) was neutral; on the twelfth, A was acid, B

was alkaline; on the fifteenth, A was acid, B was alkaline; on the sixteenth, A was alkaline, B was highly alkaline.

In disease, the number of days before the urine becomes alkaline is exceedingly variable. The average time in nineteen trials, at different periods of the year, I found was about seven days. In fever I have found the urine, which was acid when passed, become neutral in twenty-four hours; but the influence of medicine cannot be as yet separated from the effect of disease. In severe delirium I have known the urine remain acid for forty-two days after it was passed, and in some cases, this kind of decomposition appears never to take place. It is not difficult to understand one cause of the urine becoming alkaline, since the relation of urea to carbonate of ammonia has been established, and since the easy conversion of urea into carbonate of ammonia has been known; the elements present in carbonate of ammonia, and in urea and water are the same.

$$\begin{array}{ll} \text{Urea} = & C_2\,H_4\,N_2\,O_2 \\ \text{Water} = & \quad\; H_4 \quad\; O_4 \\ \hline & C_2\,H_8\,N_2\,O_6 = C_2\,O_4 + N_2\,H_8\,O_2 = \text{carbonate of ammonia.} \end{array}$$

Pure urea may be kept dissolved in distilled water, or it may, as you see in this test tube, even be boiled without being changed into carbonate of ammonia; but if a few drops of ammoniacal urine or a small quantity of mucus is added, decomposition begins. By careful experiments more may be made out on this subject than the general fact, that some substance in a state of change is requisite to cause the change in the urea to begin; and the influence of the Monads and Vibrios which are sometimes found in acid urine may be determined.

The relation of urea to carbonate of ammonia is well seen in the artificial formation of urea. Cyanate of potash, dis-

solved in alcohol, is mixed with sulphate of ammonia, and thus cyanate of ammonia forms; if this is heated in alcohol to the boiling temperature, urea is produced, and can be obtained by spontaneous evaporation in large crystals. The only difficulty that occurs is the tendency to the re-arrangement of the elements of the cyanate of ammonia, so as to form carbonate of ammonia.

Cyanic acid	$= C_2\,N\,O$
Ammonia	$= N\,H_3$
One equivalent of water . .	$= O\,H$
	$C_2\,N_2\,O_2\,H_4$ = Urea.

Whether the urine be ever secreted ammoniacal is not determined. It is a question very difficult to decide; but it is not a point of practical importance. We know that the change of the urea may take place in the bladder, and even in the pelvis of the kidney, and that this occurs most commonly in cases in which there is inflammation of the mucous membrane of the urinary organs. The mucus effused from an inflamed mucous membrane more readily effects the change of the urea into carbonate of ammonia than healthy mucus does; and in some diseases, as fracture of the spine, the mucous membrane of the urinary organs is apt to become inflamed, and thence the ammoniacal urine is formed, which, however, is by no means always found in spinal cases. There can be no inflammation of the mucous membrane without the production of pus, and sometimes the amount produced is very considerable. As long as the urine is acid, the pus-globules remain distinct, and do not adhere to one another; but when the urine becomes ammoniacal, the carbonate of ammonia slightly acts on the pus-globules, and makes them adhere together, so that a ropy gelatinous mass is formed, which includes epithelium, crystals of ammoniaco-magnesian phosphate, and granules of phosphate of lime. All these together constitute the

ropy mucus seen in cases of diseased bladder. As long as the urine in contact with the globules is acid, this ropiness is absent; but if carbonate of ammonia or any other alkali —caustic potash, for example—is added to the purulent matter taken from acid urine, the ropiness immediately appears. By agitation, air-bubbles are seen to be prevented from rising to the surface, and the liquid, when poured from one vessel to another, tails, and may be drawn into strings many inches long. As alkalies, by acting on pus, form this so-called ropy mucus, so within the bladder, if carbonate of ammonia is formed from urea before it is excreted, and if pus at the same time be present, ropy urine will be passed: the ropiness always increases after the urine has been standing for some hours.

But does the alkalescence of the urine always proceed from carbonate of ammonia? You will remember, when I spoke of the acidity of the urine, * I told you that urine passed at half-past ten A.M. may be acid, at half-past two P.M. alkaline, at half-past six P.M. highly acid, at half-past ten P.M. alkaline, and the following morning acid again. Such urine has, when alkaline, a faint, sickly smell, which has been described as resembling broth. It certainly has nothing ammoniacal in it. The alkalescence is not caused by carbonate of ammonia, but by fixed alkali—carbonate or phosphate of soda. This division of alkaline urine into ammoniacal and not ammoniacal is of some practical importance. (*Philosophical Transactions*, 1845, part ii.)

When making experiments on the solubility of urate of ammonia, I found that if blue litmus-paper was dipped into the solution, no change of colour took place, whilst the paper was wet; but as it dried, an acid reaction was distinctly shown. Solutions of other salts of ammonia gave the same results, from slight decomposition of the ammo-

* Page 47.

niacal salt. (See *Transactions* of the Chemical Society, vol. ii. p. 244.) Urine that was ammoniacal effected no change in blue litmus-paper until it dried, and then the pink colour immediately appeared. This afforded an easy method of determining whether urine was alkaline from carbonate of ammonia. The urine, which was alkaline after food, was thus tested; and I found that generally blue paper, when dry, remained blue; or pink paper became blue, and remained so. The microscope also showed a great difference between alkaline urine that was ammoniacal and alkaline urine that was not ammoniacal. The first almost always contained crystals of phosphate of ammonia and magnesia; the other rarely showed such crystals, but generally an amorphous powder, soluble in dilute acids, was seen. This was phosphate of lime. In the first, mucus and pus were usually found; in the last, mucus only, and rarely in great quantity. When the urine was ammoniacal, it was found to be constantly alkaline. It was only alkaline occasionally—that is, at particular periods of the day, when it was alkaline from fixed alkali.

If the urine be only slightly alkaline, from carbonate of ammonia or fixed alkali, no deposit may be formed on standing; yet, when boiled, a plentiful precipitate of phosphate of lime may occur; and this may sometimes happen when the urine is very slightly acid; then the acidity is observed to be increased by boiling. This results probably from the formation of a more basic phosphate of lime. The precipitate may also partly be caused by the salts of lime being less soluble in warm than in cold water. Any urine containing earthy phosphates can be made to give this precipitate by heat, simply by lessening its acid reaction. Here, in these test-tubes, are two portions of the same healthy urine. I will slightly lessen the acidity in one only; and now you see, on boiling both, the portion which

is least acid gives a plentiful precipitate by heat; whilst the portion in the other test-tube remains perfectly clear. The precipitation of the phosphates of lime and magnesia, with heat or without heat, is chiefly, if not entirely, caused by the removal or diminution of the acidity of the urine. It has little to do with the excess of these phosphates in the urine. The cause of the appearance of the earthy phosphates is the absence of the acid or the presence of the alkali, whether volatile or fixed.

When on the variations of the acidity of the urine, I pointed out to you how the alkalescence from fixed alkali was produced; and here I may repeat, that it requires only to be carefully looked for to be found. Dr. Andrews, of Belfast, a most excellent chemist, told me that he examined the urine, immediately after it was voided, about noon, by fifteen students of his class in good health, and that he found it was alkaline in about two-thirds of the cases. When the urine is alkaline from fixed alkali, and but little urate of ammonia or other ammoniacal salt is present in it, then phosphate of lime is the only precipitate. No prismatic crystals of phosphate of ammonia and magnesia are seen. To form this compound ammonia must be present, and hence it is that these crystals of triple phosphate are most sure to occur when the urine is alkaline from carbonate of ammonia. Phosphate of magnesia is a constituent of healthy urine, but phosphate of ammonia and magnesia is not so, though occasionally it is found to be present, and it may be in such excess that prismatic crystals may form even in very feebly acid urine. Thus, even in feebly acid urine this ammoniaco-magnesian phosphate may, in some rare cases, be found, depending probably, in part, on an excess of this substance in the urine. If the urine is highly acid, there will be no appearance of prismatic crystals, even when a great excess of this phosphate is present.

If the urine is highly alkaline from ammonia, a very small amount of phosphate of ammonia and magnesia will give a perceptible precipitate; and the same is true regarding the precipitate of granular phosphate of lime, when the urine is alkaline from fixed alkali. If it be highly alkaline, a much greater precipitate of phosphate of lime will occur than when the urine is neutral or very slightly alkaline; and the urine that gives least precipitate does not always contain least phosphate of lime.

We come therefore, here, to the same conclusion as we did regarding the precipitation of urate of ammonia—that the precipitation is dependent on two causes, which usually, by acting together, produced the result—first, the degree of alkalescence of the urine; second, the amount of earthy phosphates present. The first is the most important and the most efficient cause; and, as a general rule, the precipitation of the phosphates simply indicates that the urine is alkaline, and, for the most part, if granular crystals, soluble in dilute acids, are seen, the urine is alkaline from fixed alkali. If prismatic crystals, soluble in dilute acids, occur, the urine is alkaline from carbonate of ammonia. Shortly, I might state that the first is produced by indigestion, and is variable; while the second is the effect of inflammation, and is much more constant.

The following comparison of these two states, which you see in these two specimens of urine, will make their distinction more clear.

Alkalescence from carbonate of ammonia caused by local disease.	Alkalescence from fixed alkali caused by general disorder.
Blue paper made red on drying.	Blue paper not made red on drying.
Alkalescence constant.	Alkalescence variable.
Excess of mucus and pus present.	No pus. Rarely much mucus.
Prismatic crystals generally seen.	When first made, granular deposit only seen.
The iridescent film has prismatic crystals.	The iridescent film consists of thin plates only.

As ammoniacal urine is only the result of metataxis, or re-arrangement of the elements of the urea, I shall, not inappropriately, at the conclusion of this lecture, occupy your attention with the subject of urea.

This substance is formed, not only in the kidneys, but in the system. It exists in healthy blood in small quantity. Even in the aqueous humour of the eye it has been found. The kidneys have been removed from animals, and then an excess of urea has been found in the blood. If the secretion of urine is suppressed, as in cholera, or hindered, as in albuminous urine or inflammation of the kidney, or obstruction of the ureter, perhaps even of the urethra, urea accumulates in the blood.

In its chemical properties urea is closely related to the alkaloids. It is an alkaloid with slightly poisonous properties, but without alkaline reaction. It combines with acids. The nitrate of urea and oxalate of urea are the most important compounds for us. From the nitrate of urea the quantity present in the urine has generally been calculated, but the composition of this compound has, till lately, been uncertain. The most trustworthy numbers are those of Regnault.

Urea . .	48.8
Nitric acid and water.	51.2
	100.0 nitrate of urea—that is, every 100 parts of dry nitrate of urea contain nearly 49 parts of urea.

I have already told you that urea can be formed artificially from the cyanate of ammonia, but you must not suppose, from its relation to cyanogen, that it is highly poisonous. In small quantities it does not cause death. In large doses it seems to act, like hydrocyanic acid, as a pure sedative, producing coma by stopping sensation and excitomo-

tory power. It may, and probably does, deprive the muscular structure of its power of shortening, and it appears to hinder the coagulation of the fibrin of the blood, causing it to remain fluid after death, and to coagulate imperfectly, if taken from the body. during life. Accurate experiments on all these points are wanted, and would be very valuable.

Urea may be formed in many ways; of these, a summary is given in the volume of papers published by the Cavendish Society.

It has not yet been formed out of the body, as it probably is formed in the body by an action of oxygen on the tissues of the body. The substance from which it is formed is not yet perfectly clear. It may be from kreatine. It is not impossible that, as in the laboratory, by the action of oxygen on uric acid, urea is formed, so in the body, some of the urea may be formed out of the uric acid that exists in the blood, but this is by no means yet proved to be the case.

The quantity of urea in the urine is subject to great variations. It is probably most after food, but I have not as yet traced the variations it presents, because it is very difficult to do so accurately. There are many ways of detecting the presence of urea, but as yet there is no way of easily and accurately determining the quantity present. It is much easier to point out objections to, than to tell the advantages of, any method of quantitative analysis of the amount of urea. No doubt the best process is that recommended by Professor Bunsen, but it is not easy. The nitric-acid process is not fit to be trusted for accurate results. Professor Bunsen's method consists in introducing a weighed quantity of urine into a strong glass tube, with caustic potash, sealing it up, and keeping it at a high temperature in an oil-bath. The quantity of ammonia produced is determined in the ordinary way by chloride of platinum.

An easy, though a very rough way of estimating the quantity of urea, is to place a single drop of urine on a slip of glass, leaving it to evaporate to perfect dryness, when, if much urea is present, long crystalline streaks of urea will be seen to have crystallized out on the glass. This can be done quickly over sulphuric acid, and for a trial test it may be sufficient, provided the urine be acid. M. Millon has very lately suggested a new method, very accurate and very difficult. He found the urea varied in the same person from four grains per 1000 grains urine, specific gravity 1004.6, to 29.72 grains per 1000 grains urine, specific gravity 1030.8. He gives the following table, showing how equal quantities of different substances dissolved in the same quantity of water, give solutions of different specific gravities :—

40.289 grms. urea,	dissolved	in 1000 grms.	water at 60°.	Spec. gr.	= 1010.4
20.180	,,	,,	,,	,,	= 1005.3
10.078	,,	,,	,,	,,	= 1001.6
40.311 grms. common salt	,,	,,	,,	,,	= 1024.1
20.063	,,	,,	,,	,,	= 1011.9
10.031	,,	,,	,,	,,	= 1006.0
40.267 grms. sulph. of potash	,,		,,	,,	= 1029.7
20.087	,,	,,	,,	,,	= 1014.0
10.055	,,	,,	,,	,,	= 1007.1

This is positive proof that the specific gravity is no index to the quantity of solid matter in the urine. From his experiments, it appears, however, that frequently in health the specific gravity bears such a relation to the amount of urea, that by determining the former, the quantity of urea in 1000 grains of urine may be rightly given. Thus—

Specific gravity	= 1004.6	urea per	1000 grs. urine	= 4.39
,,	= 1009.2	,,	,,	= 9.88
,,	= 1011.0	,,	,,	= 10.60
,,	= 1011.6	,,	,,	= 11.39
,,	= 1014.3	,,	,,	= 11.99
,,	= 1026.0	,,	,,	= 25.80
,,	= 1027.7	,,	,,	= 29.72
,,	= 1029.0	,,	,,	= 31.78

But this, he shows, is by no means always the fact, more particularly in disease, therefore you can never trust to this mode of guessing at the amount of urea in the urine.

Dr. Prout, many years ago, pointed out two states of urine —the one containing urea in excess, and the other a deficiency of that principle. No progress has been made in tracing their history. Both states appear to me to be symptoms or effects of some other better known disorders. Thus excess of urea occurs in some cases of indigestion in which oxalate of lime exists in the urine, and in some cases of delirium tremens and chorea; while deficiency of urea accompanies ammoniacal urine and diuresis and chronic inflammation of the kidneys.

But as yet it is in its decomposition that urea is of most importance to us, and the ammoniacal urine that results therefrom, admits of very little purely chemical treatment. Everything depends on the removal of the cause of the change in the urea—that is, we must direct our treatment chiefly to the inflammatory action which is going on in the mucous membrane of the urinary organs. We shall, however, gain much in clearness of diagnosis, if not in treatment, by carefully separating ammoniacal urine from urine that is alkaline from fixed alkali; and both these states from the true phosphoric diathesis, in which an excess of alkaline and earthy phosphates is excreted.

As regards the treatment of alkalescence of the urine from fixed alkali, as it depends on irritability of the stomach, we must treat the stomach and not the kidneys, using the state of the urine only as an index of the state of the stomach, and to this our remedy must be directed, and not to the state of the urine. Even the mineral acids, though they act chemically on the urine, appear to me to act more importantly on the stomach.

In treating chronic irritation or inflammation of the

stomach, and indeed of all the mucous membranes, the employment of sedatives and of stimulating medicines is a question of time and degree, for which no general rule can be given.

That food which causes least irritation to the stomach, mechanically and chemically, should always be chosen ; and whatever medicine is given to act on the stomach should be given previous to food, for otherwise the medicine acts on the food and not on the stomach.

LECTURE X.

ON ALBUMINOUS URINE.

The states of the urine of which I have hitherto spoken, have, with the sole exception of ammoniacal urine, been quantitative changes; that is, excess or deficiency of substances which occur in the urine of health. I come now to qualitative changes, or the presence of substances which, in the state of health, are never to be found in the urine.

In health, albumen is not a constituent of the urine. Healthy urine, when boiled, never gives a precipitate that is insoluble in a drop or two of nitric acid. I have no doubt that very many of the statements regarding the presence of albumen after food, and in the convalescence from severe diseases, arose from the first half of the test—that is, the effect of heat—being alone employed. The time when the urine is most alkaline, and the earthy phosphates most apt to be precipitated by heat, is after indigestible food, or when the stomach is weak and irritable during convalescence. Up to the time, and even at the commencement, of Dr. Bright's researches, heat or more rarely nitric acid, were separately used as tests of albumen; and Dr. Owen Rees tells me that heat and acid together were first employed by him when making experiments for Dr. Bright

Albumen is much more soluble in hydrochloric than in

nitric acid, and on this account I always prefer the latter acid. Nitric acid without heat is a very delicate test for albumen, and if nitric acid never precipitated anything else, or if albuminous urine were only a solution of albumen in distilled water, nitric acid alone would be a sufficient test; but there are other substances in urine which may be precipitated by nitric acid besides albumen. There may be urate of ammonia, and even urea, present in excess, and the addition of nitric acid may throw down a precipitate of urate of ammonia, or nitrate of urea, and the eye alone cannot, without some practice, see the difference between a precipitate of urate of ammonia or nitrate of urea, and a precipitate of albumen and nitric acid. After precipitation by nitric acid, the effect of heat on the precipitates is very different; the urate of ammonia and nitrate of urea are soluble by heat; the nitrate of albumen, in dilute nitric acid, is insoluble when heated; other substances—cubebs, for example, are said to give a precipitate with nitric acid alone. Hence the fallacy of the nitric-acid test alone is, that it may give a precipitate when albumen is absent. I know only of one case in which it gives no precipitate when an albuminous substance is present, and this is not likely to occur. Hence you may take as a rule—if nitric acid gives no precipitate, little or no albumen can be present. I therefore, for quickness, generally try nitric acid first. If no precipitate falls, or forms on standing for a few seconds, I decide gainst the presence of albumen. If there be a precipitate, I boil the same acidulated specimen of urine. If the precipitate be permanent on boiling, I consider the presence of albumen highly probable. Before I decide, I clean the tube, and boil the urine first, and then add a drop or two of nitric acid. The object of cleaning the tube is, to free it perfectly from acid, for a small quantity of acid hinders the coagulation of albumen by heat. Here, as you see, is some

urine which coagulates on the addition of heat and acid; on throwing this out of the tube, and adding some more of this urine, without cleaning the tube, you will see that the small quantity of nitric acid which remains will hinder the coagulation of the urine by heat. And now, if I boil the urine no precipitation occurs. I have many times known an unclean test-tube, containing merely a trace of strong acid, lead to the assertion that urine contained no albumen, when a very considerable quantity really was present. The urine of a patient in St. George's Hospital was highly acid, and gave a precipitate with nitric acid, which was not re-dissolved when heated. This urine would not coagulate with heat alone. I thence concluded that the precipitate with nitric acid was not albumen, nor was it until many months afterwards that I found that very acid urine might contain albumen, and yet not coagulate with heat alone. (See a paper in the *Medical Gazette* for Nov. 13, 1840, On some Properties of a Combination of Albumen with Acids.) A very small quantity of acid is quite sufficient to stop the appearance of all evidence of albumen, for the compound of the albumen with the acid is soluble in cold and boiling water, though it is quite insoluble in cold or boiling dilute acid. The addition of the acid after the urine is boiled, is sure to prevent the albumen from being overlooked. Alkaline urine also, as well as very acid albuminous urine, will not coagulate by heat, for alkalies, volatile or fixed, hinder the coagulation of albumen by heat.

Instead of filling the test-tube with urine, it is well to take about a drachm of urine, filtered, if it be not previously quite clear—that is, if any cloudiness at all is perceptible the urine ought to be first filtered; then the clear liquid should be boiled for a minute, and afterwards two drops of nitric acid should be added.

If a very great excess of nitric acid is added, (and fre-

quently a volume of nitric acid is added, equal to the bulk of urine,) then the albumen may be re-dissolved, and perhaps this happens more readily if an excess of common salt or other chloride is present, but this solution, in strong acid, is rendered cloudy by the addition of distilled water. Moreover, sometimes albumen that is soluble in strong nitric acid when heated, is precipitated again on cooling.

Other tests have been mentioned. A beautiful one is the purple colour that albumen forms with sulphate of copper and liquor potassæ. Gelatine, however, gives the same colour.

Very strong hydrochloric acid gives a most beautiful purple colour, with dry, albuminous substances.

A very delicate test is ferrocyanide of potassium, after the urine has been made acid with acetic acid.

The evaporation of a drop of urine on a slip of glass, placed on a water-bath, is also a ready means of detecting the presence of albumen. If any albumen is present, on perfect evaporation, it adheres so firmly to the glass, that it is by no means easy to clean it. This evaporation may also be effected in a watch-glass, over a spirit-lamp, in a few seconds, care being taken to hinder the urine from boiling, by holding it far from the flame.

If, by these, or any other method, we satisfy ourselves that albumen exists in the urine, what does it indicate?

The answer to this question is, that, by itself, albumen in the urine indicates only excessive congestion of some part of the urinary organs; but albumen rarely, if ever, occurs in the urine, without some other substance being also present, by which we may determine the cause of the presence of the albumen.

Blood-globules, fibrin, or pus-globules, are the three substances, one or other of which usually are present with the albumen, and by which the cause of its presence may, with

tolerable certainty, be conjectured. The quantity of albumen is often of less importance than the quality of the substances which accompany it.

Having made sure of the presence of the albumen, by chemical means, the microscope is absolutely necessary for determining whether blood-globules, fibrin, or pus-globules are also to be found.

The urine, in a phial, should be left to rest for twelve hours. If many blood-globules are present, a remarkably distinct red line of globules will form at the bottom of the bottle. Thus, sometimes blood may be seen, which could not otherwise be recognised. If but very few blood-globules are present, the eye will not perceive them, unless a drop of the deposit is examined by the microscope. It very rarely happens that blood-globules are found by the microscope, when no albumen can be detected by heat and acid. This arises, sometimes, from the solution of the albumen being excessively dilute; so that evaporation in vacuo over sulphuric acid is requisite to get a more concentrated solution, which then will give a precipitate with heat and acid. Still more rarely, the blood-globules may be washed out of some small coagulum, long after all the more soluble albumen has been removed,—just as in washing the clot from blood the albuminous fluid is first and quickly removed; and long after the albumen is entirely gone, some blood-globules will still be capable of separation by further washing.

The fibrin can be seen solely by the microscope. It it be moulded into the urinary ducts of the kidney, and if albumen is also present, with or without blood-globules, you may be quite certain that there must be congestion of the cortical structure of the kidneys, and most probably the disease is Bright's disease, or the result of scarlet fever. The history and accompanying symptoms enable you to perfect your diagnosis.

If blood-globules are found, and no fibrinous casts, it is most probable that the congestion is not in the secreting structure of the kidney; and if crystals of uric acid or oxalate of lime are seen, then probably some calculus is causing abrasion of the vessels of the mucous membrane. Whether the calculus is in the pelvis of the kidney, the ureter, the bladder, or prostate, must be determined by the general symptoms. The nature of the epithelium that occurs in the urine will also sometimes help to determine this question. The occurrence of the blood only on exertion, tends to confirm the diagnosis of a calculus.

If pus-globules are found with albumen, you may be certain that suppurative inflammation is going on; if, at the same time, fibrinous moulds are seen, then probably Bright's disease and inflammation of the pelvis co-exist. If there are no moulds, but blood-globules and pus-globules, then probably a calculus has caused not only abrasion, but inflammation, of the mucous membrane. When fibrinous moulds, blood-globules, pus-globules, albumen, and crystalline deposit were seen, then degeneration of the kidney, inflammation, and calculus, were found on post-mortem examination.

But you will say, how can we distinguish pus from mucous globules? The appearance of well-formed pus and mucous globules is very different: in pus-globules active nuclei are almost always to be found—that is, young pus-cells growing within the old ones. Mucus does not thus multiply; the cell is slightly granular only. With mucus, exudation globules do not occur; with pus, they are constantly found.

Ropiness with alkalies is not peculiar to pus, but mucus is very rarely present in sufficient quantity to admit of this action of alkalies.

Pus globules also appear to have a higher specific gravity than mucous globules; on standing in a phial with urine

the pus falls in a distinctly bounded sediment; the mucus forms a cloudiness, with much less defined border.

These are the best diagnostic marks I can give you; constant practice in looking at such sediments will be of great assistance; but even to the most practised eye slight alterations of the pus and mucus will hinder you, in some rare cases, from being perfectly certain which you are looking at, but by watching the urine made on different days for a short time you will not long be in doubt. If no trace of albumen is present, the globules must be mucus, and cannot be pus; for pus consists of cells in an albuminous fluid. The quantity of albuminous fluid may be very small indeed, and the number of cells may be very great, but if pus be present there must always be a trace of albumen detectible, on careful examination. The quantity of albumen will vary with the intensity of the inflammation, that is, with the degree of congestion that is present.

The determination of the presence or absence of albumen in the urine is of the greatest importance, because if it be not found, neither fibrin, pus, nor blood, can, as a general rule, be present. Thus, by determining the absence of the albumen, we may infer the absence of blood-globules, fibrin, and pus, and we may conclude that neither Bright's disease, calculus, nor inflammatory action in the mucous membrane is present; but when the presence of albumen is determined, no deduction can safely be drawn from this fact, until the further question is answered, as to the presence of the fibrin, blood, or pus. By these, the meaning of the albumen can be known with tolerable certainty, and the treatment must, in great measure, be determined by the conclusions we thus arrive at, regarding the presence of disorganization, mechanical abrasion, or inflammation.

There is another cause of albumen in the urine, which, as it nearly led me into a mistake, I may here mention to you.

Usually, spermatozoa are found in urine, which is perfectly free from all trace of albumen. On examining the water of a patient, passed early one morning, I found it to be highly albuminous by heat and acid. The microscope showed multitudes of seminal animalcules. As I never before had found such urine albuminous, I asked to have the urine made late at night, and early the following morning. The night urine was thick, from urate of ammonia, but contained no albumen. The morning urine gave a pinkish albuminous precipitate, and contained spermatozoa. The urine passed the following morning contained neither albumen nor spermatozoa, and I have not again found the albumen. The brother of this patient is said to have died of dropsy and diseased kidney.

I might occupy your time for many lectures with disorganization of the kidneys; calculus; inflammation of the bladder; congestion of the kidneys, from diseased heart, from scarlet fever, from cholera, and from pregnancy, all of which are characterized or accompanied by albumen in the urine. But these subjects belong more to lectures on medicine than to lectures on chemistry.

I shall, in conclusion, direct your attention to a new albuminous substance, which you see precipitated here from the urine by alcohol. The urine that contained it did not give a precipitate immediately by nitric acid, and when heated it did not coagulate, and nitric acid, when added to the boiling urine, did not give a precipitate. If, after boiling, the urine was cooled, then the precipitate fell, but was immediately redissolved by heat. Thus, then, neither heat and acid, nor acid and heat, gave an immediate precipitate, and yet this urine was as loaded with an albuminous substance as the serum of the blood usually is. In fact, this peculiar substance is not albumen, but it is closely related in composition to ordinary albumen. The same substance

has been found in the buffy coat of inflamed blood, and it may also be met with in the albuminous fluid of pus. Its nature and composition you will see in detail in the *Philosophical Transactions* for 1847. On post-morten examination, the case proved to be one of mollities ossium.

[Since this lecture was given, I have had the opportunity of carefully watching for some months another rare case of albuminous urine. The patient had for some time been passing the so-called chylous urine, but I have fully satisfied myself that the presence of fat in the urine, on which the white colour depends, is an accident only, and by no means the most peculiar part of the disease.

The fat passes off in the urine, making it milky only after food; but previous to any food, albumen, fibrin, alkaline salts, and more rarely a few blood-globules, are thrown out by the kidneys, and their appearance in the urine depends on the increased force of the circulation, consequent on waking, on getting up, and on exertion; for during sleep, the albumen, fibrin, &c., entirely disappear from the urine, and by directing the patient to lie in bed, or to get up and exert himself, totally different kinds of urine were passed. When perfectly quiet, the albumen and fibrin were prevented from appearing in the urine, and when he exerted himself before breakfast these substances transuded in very considerable quantity. The urine sometimes, previous to any food, even formed a jelly-like clot, transparent, and perfectly free from the appearance of fat. I satisfied myself that the disease does not depend on chyle or on fat, but on a slight alteration in the state of the kidney, and on the circulation of blood through it. Under treatment the urine became perfectly healthy.]

There is a peculiar microscopic appearance in acid albuminous fluids, to which M. Andral has directed attention. In the "Annales de Chemie," vol. lxxxiii. p. 385, there is a

paper on the development of the penicilium glaucum, under the influence of acidification, in the albuminous fluids of health and disease, by MM. Andral and Gavarret. Serum of the blood diluted with twice its volume of water, and acidified by dilute sulphuric acid, usually, in twelve hours, gave vesicles, which elongate rapidly, forming a long, branching, jointed vegetable, of which drawings are given in the different stages of its development; albumen and acid are necessary for its growth; if, therefore, this vegetation is met with in the urine, we may immediately conclude that albumen exists in solution, and heat and nitric acid will certainly confirm the truth of your opinion.

I will, finally, say a few words on the peculiar microscopic appearances of the urine in Bright's disease. In the *Medical Gazette* for June 13th, 1845, I have given the history of the discovery of the fibrinous casts or moulds of the ducts, and the following summary of the appearances I had observed:—1st. Ordinary mucous globules. 2nd. Laminar epithelium. 3rd. Blood globules. 4th. Round, dark globules filled with granular substance, in diameter varying from $\frac{1}{1666}$ of an inch to $\frac{1}{1111}$ of an inch. 5th. Tubes or amorphous matter adhering in the form of tubes (like fibrin). These tubes, for the most part, have an actual covering, and are cylindrical, as may be easily seen by causing them to revolve in the urine. Sometimes the covering is wanting, and then an amorphous slightly granular matter adhering like a cylinder is seen. The true cylinders are sometimes quite full, sometimes empty, and containing only a small quantity of granular substance. The cylinders, which are full, contain sometimes light, and at other times a darker granular matter, which consists of small nuclei among which cells and globules, similar to mucous globules, appeared to exist, but I was not able on all occasions to convince myself of their presence (within the cylinders). The breadth

of the cylinders varied, the broadest I estimated at $\frac{1}{900}$ of an inch, the smallest ones at $\frac{1}{1666}$ of an inch. After scarlet fever, in cholera, and in congestion of the cortical structure of the kidney, the same cylinders may be found to be present in the urine.

LECTURE XI.

ON DIABETES AND DIURESIS.

The formation of sugar by animals is of no rare occurrence. I have already told you, that in the process of digestion in health, starch is changed into sugar. Even in healthy secretions sugar may exist—as in milk, for example—and no disease, no injury to the health, be indicated thereby. Milk sugar differs indeed slightly from cane sugar or grape sugar. It distinctly differs in sweetness and in the acids that can be formed from it; but in composition it is nearly identical with diabetic sugar. The quantity of sugar present in milk varies considerably. In human milk it exists to the amount of six and a half per cent. (*Journal de Pharmacie*, xxv. pp. 333, 401.) Other glands besides the mammary glands may contain sugar; thus, it has been said that the liver always contains sugar, though the bile contains none.

The excretion of sugar by the kidneys constitutes the disease which is called diabetes. The amount of sugar in the urine is frequently less than is met with in milk; but the important point with the medical man is the determination of the presence of the sugar, and not the quantity in which it exists. The proof of the existence of sugar in the urine is of primary importance; the question of how much sugar

is comparatively of very little consequence. Hence the tests for the sugar in the urine are of more value than the methods of making quantitative analyses of it.

All the tests depend on the facility with which the composition of the sugar is changed. For the medical man the most valuable test is the action of some metallic oxides, aided by heat and alkali, on the elements of the sugar. Oxide of copper and oxide of silver are the most worthy of your attention.

To a drachm of suspected urine, add two or three drops of a saturated solution of sulphate of copper, and then two drachms of caustic potash; the hydrated oxide of copper, which first is precipitated, re-dissolves if sugar or many other organic substances are present, becoming of an intense blue, and when heat is applied, if grape sugar be present, the oxide of copper is rapidly reduced, and reddish-yellow suboxide of copper is precipitated.

The reduction of the oxide of copper is effected by the carbon of the sugar; perhaps I shall make this more clear to you by the following diagram:—

$$\left.\begin{array}{l}\left.\begin{array}{r}\text{Sulphate}\\ \text{of copper}\end{array}\right\} = SO_3CuO,\ HO+4HO\\ \quad\text{Potash} = KO \ \ldots\ldots\ldots\ldots\end{array}\right\} = \left\{\begin{array}{l}SO_3,\ KO+4HO = \left\{\begin{array}{l}\text{Sulphate}\\ \text{of potash.}\end{array}\right.\\ Cu\,O,H\,O = \left\{\begin{array}{l}\text{Hydrated oxide}\\ \text{of copper.}\end{array}\right.\end{array}\right.$$

$$\left.\begin{array}{l}\text{Grape sugar} = C_{24}\,H_{28}\,O_{28} = C_{24}\ 28\ HO\\ \left.\begin{array}{l}\text{96 hydrated}\\ \text{oxide of}\\ \text{copper}\end{array}\right\} = \left\{\begin{array}{l}Cu_{96}\ O_{96}\\ 96\ HO\end{array}\right\} = \left\{\begin{array}{l}Cu_{96}\ O_{48}\\ O_{48}\,96\,HO\end{array}\right.\end{array}\right\} = \left\{\begin{array}{l}C_{24}\ O_{48}+124\ HO = 24\ \text{Carbonic acid}\ 124\ \text{water.}\\ Cu_{96}\ O_{48} = \left\{\begin{array}{l}\text{96 Sub-oxide of}\\ \text{copper.}\end{array}\right.\end{array}\right.$$

If cane sugar instead of grape sugar be used for this experiment, the rapid reduction of the oxide to the suboxide will not take place; the bright blue solution will be formed, but the heat must be long continued to effect the reduction; uric acid and albumen will also effect a slow reduction of the oxide of copper. You will from this see that it is the rapidity, with which the grape sugar is decomposed, that

constitutes the test; and hence it has led, and may again lead, to many mistakes. It is always well to repeat the experiment, if it be an unsuccessful trial, with more or less sulphate of copper than was used in the first experiment. If very little sugar is present, too much sulphate of copper hinders the reaction from being clearly seen; if very much sugar is present, the reaction will come out more distinctly by adding more sulphate of copper.

The reduction of oxide of silver by grape sugar and cane sugar is beautiful, and though not quite so easy a test as the oxide-of-copper test for diabetes, yet it may be often used as an additional proof where any doubt exists. A saturated solution of nitrate of silver is made—a few drops of this are to be placed in a test-tube, and a single drop of caustic ammonia is to be added; if a brownish oxide of silver falls, a single drop of the suspected urine is to be added, and the test-tube is then to be heated, and the contents to be well shaken. In a few seconds the sides of the tube will be coated with silver, and the metallic lustre will be seen. The carbon of the sugar will take the oxygen from the oxide of silver. Carbonic acid and metallic silver will be produced.

Potash alone decomposes grape sugar, and so it has been used as a test of diabetes. When heat is applied to urine containing grape sugar and liquor potassæ, the sugar is decomposed into formic, sacchulmic, melassic acids, and thence a great change in colour results; no precipitate forms, and on this account the test ought not solely to be relied on, as some deepening of colour may ensue when no sugar is in the urine. Fermentation by means of yeast is the oldest and surest evidence of the presence of sugar, and by this means the quantity of sugar present is best determined, either by the loss of carbonic acid, or by determination of the quantity of alcohol produced.

Another easy and satisfactory test is the appearance of the residue of the urine after a drop is evaporated on a slip of glass to perfect dryness. This can be done rapidly over sulphuric acid by placing a slip of glass on a small capsule of strong sulphuric acid, and covering the whole with a basin. When the glass becomes dry, granular specks are seen, which, if examined by the microscope, appear to be tufts of crystals, most probably a compound of urea and sugar. If the atmosphere is very moist, these crystalline granules rapidly deliquesce. If by two or more of these tests the presence of sugar is established, it is not necessary to make any quantitative analysis; for the medical man a sufficiently near approximation is obtained by taking the specific gravity of the urine. It is not even necessary to refer to Dr. Henry's tables, in which he gives the solid contents of diabetic urine of all specific gravities. All the information you can use you will obtain by comparing, from day to day, or from week to week, the specific gravity and the total quantity of urine passed in twenty-four hours. The specific gravity will rise or fall according as the sugar is increased or diminished if the quantity of water is fixed.

If the urine contains albumen also, the test for sugar is more uncertain, and it can in doubtful cases only be made sure by evaporating the urine to dryness, reducing it to a fine powder, and treating it with boiling water, and then filtering it. The sugar is dissolved, the albumen remains undissolved. In the 26th volume of the *Medico-Chirurgical Transactions* you will see that sugar may thus be detected in the serum of the blood.

Now, regarding the frequency of the occurrence of diabetes, it is by no means so rare as has been said; the delicate tests we possess will discover it to be a not unfrequent disease, long before the ordinary symptoms of the complaint appear. Most of the cases at present recorded or observed

are those in which the disease has been fully formed; and yet, from Dr. Prout's great experience, we can see that even such cases are very frequent. In the edition of

1821, Dr. Prout gives no cases of his own.

1825, twenty cases; the average of four years was five cases a year.

1840, no statement is made.

1843, five hundred cases; the average of eighteen years was twenty-seven cases a year.

1848, seven hundred cases; the average of five years was forty cases a year.

In all these cases the disease was probably fully established, and hence perhaps the statement as to the incurability of the disease. So far as can be discovered, there is no organic disease which renders cure impossible. Extreme disorder of functions is all that can be made out. Altered chemical action constitutes the disease, and yet chemical investigations have not enabled us to discover the remedy. One cause of this is, that at present the theory of the disease is unknown. I shall attempt to show you the present state of our knowledge on this question, and give you the explanation of the disease, which at present appears to me most probable.

You will remember, in my lecture on Digestion, I traced the course of a grain of starch, in its chemical changes, passing through dextrin, sugar, and vegetable acid, to carbonic acid. The greater portion of the starch we take passes into dextrin; all the dextrin becomes sugar, and all the sugar is converted into vegetable acid previous to its being oxidized into carbonic acid. The changes which occur in health may be represented by the following series—starch, dextrin, sugar, vegetable acid, carbonic acid. There may be many intervening terms in this series, but these are suffi-

cient to show the direction and nature of the chemical changes which occur in the body.

The disease—diabetes, I consider arises from the arrest or stoppage of these healthy and necessary changes. The series of changes is stopped at the sugar; from some cause the conversion of the sugar into vegetable acid and carbonic acid does not take place, and the whole series of changes may be indicated by the terms starch, dextrin, and sugar. *

When speaking to you of uric-acid crystals in the urine, I pointed out how these were caused by an excess of free acid in the urine, which, reacting on the urate of ammonia or soda, combined with the ammonia or soda, and liberated the uric acid. I have carefully watched some of these cases of excessive acidity of the urine; and I am led to think that there is a peculiar disease, which shows itself by free acid constantly passing off in the urine; and that this disease is very nearly related to diabetes. The existence of free acid in the urine, and the existence of sugar in the urine, are both results of one cause—the arrest of the chemical changes which, in the state of health, take place in the human body. The changes proceed further in the disease which is characterized by excessive acidity, than in that characterized by sugar in the urine. Thus, in excessive acidity, the series of changes may be represented by starch, dextrin, sugar, and vegetable acid.

* Though the greater part of the sugar in the urine comes from the starch taken as food, yet the following facts indicate that there is probably another cause also acting in this disease:—

1st. When diabetes is fully established, total abstinence from starch and sugar will not entirely stop the appearance of sugar in the urine, although by this means the amount of sugar may be very greatly diminished.

2nd. In the milk of carnivorous animals, fed exclusively on meat, sugar of milk, from the researches of Dr. Bensch, (" Annalen der Chemie und Pharmacie," vol. li. p. 221,) is invariably present.

3rd. By boiling with water, the livers of animals fed exclusively on flesh have furnished positive proof of sugar. This fact has now been confirmed by many observers.

We may, then, arrange these diseases, and show their relation to the healthy state, and to each other, thus :—

In health, the changes are, starch, dextrin, sugar, vegetable acid, carbonic acid.

In excessive acidity, starch, dextrin, sugar, vegetable acid.

In diabetes, starch, dextrin, sugar.

I have some reason for thinking that other terms will be found—other stages, through which the starch passes in its conversion into carbonic acid and water.

Between dextrin and diabetic sugar another substance has occasionally been met with by good chemists ; and as it will add something to the clearness of your view of diabetes, I shall shortly tell you what is known regarding it. You have all probably heard of diabetes insipidus—the term has been applied to all cases in which a large quantity of urine, free from sweet sugar, has been passed. The history of diabetes insipidus will show you how incorrectly this term has been used.

In 1806, M. Dupuytren and Thenard, in the *Annales de Chemie*, vol. lix. p. 41, had a patient making a large quantity of urine, feebly acid, slightly sweet, of somewhat saline taste, which,on evaporation, left granules that had scarcely any sweetness. It was concluded that this granular substance contained only a small quantity of sugar. Nitric acid, alcohol, and yeast, had, however, the same action on this substance as on sugar. The conclusion, therefore, was, that though much less sweet than sugar, yet in some way this granular substance was identical with sugar. The patient was kept from all vegetable food, and he was dismissed cured, but he relapsed, the diabetes reappearing with other diseases, and he died.

Thenard says, in his "Chemistry," of this same case, vol. iii. p. 177, edit. 1818 : "I have had occasion to extract from the urine of a diabetic patient of M. Dupuytren's fifteen

killogrammes (thirty-three pounds) of the nearly tasteless sugar, so little sweet that it might be taken for a kind of gum. It dissolved in water; and when in contact with yeast, ferments as well as the other kind of diabetic sugar does: whence it is evidently a kind of sugar, though a distinct species."

Bourchardat, in the *Revue Médicale*, June, 1839, writes thus:—

"The division into insipid and sweet diabetes I adopt, without attaching the least importance to it, because the urine of the same patient may contain successively tasteless and sweet sugar. I have examined the urine in three cases which contained tasteless sugar. They presented, in a very slight degree, the symptoms of diabetes. Thirst, appetite, and quantity of water, were moderate. If the urine is evaporated over a fire, a black extractive matter, without any of the external characters of sugar, remains; but at a lower temperature (140° F.) a clear liquid is obtained, which, at 68° F., gives crystals of tasteless sugar, capable of undergoing alcoholic fermentation."

He continues,—

"At the commencement of my researches I thought that diabetic urine contained very rarely this variety of crystalline tasteless sugar, but nothing is more common. Most patients with sweet diabetes, who live on animal or vegetable diet, pass urine which contains this tasteless sugar. It crystallizes exactly like grape sugar. It differs only in its taste, which is perfectly insipid, not only when crystallized, but when in solution: when fermented it gives the same quantity of carbonic acid and alcohol as grape sugar. Alkalies have the same action on it as on grape sugar, blackening it. Acids, when cold, effect no change in it, but if boiled for ten hours in water, acidulated with one-tenth of sulphuric acid, this insipid is changed into sweet sugar, which can be crys-

tallized. The composition is the same as that of sweet diabetic sugar. The insipid and the sweet sugar are two isomeric bodies. I will elsewhere give the details of the analyses.

"It is a curious fact, that this intermediate body, which resembles dextrin in its insipidity and property of becoming sweet when boiled with acids, and which differs from dextrin in its crystallization and solubility in alcohol, and in immediately being capable of undergoing alcoholic fermentation, is a substance which we cannot prepare in the laboratory, and which hitherto has only been made under the influence of organization, in this resembling most closely milk sugar."

Dr. Simon says, (p. 454, German edition,) "I once had an opportunity of seeing such sugar in the urine. A young woman with diabetes, eight weeks before her death, was passing a large quantity of very saccharine urine, specific gravity = 1032. The sugar, when separated, had all the properties of grape sugar. She became much weaker, and two days before her death the urine specific gravity = 1021, was again sent for examination, and I was not a little astonished to find a perfectly tasteless sugar, soluble in hot alcohol; there was mixed with it a considerable quantity of a substance more like gum, which was insoluble in alcohol, and when heated had a peculiar smell."

I have given these details, partly that you may have a clear idea of diabetes insipidus, but still more because it is probable that, by the re-investigation of this insipid sugar, something will be added to the true theory of diabetes. In insipidity this insipid sugar resembles sugar of milk; it differs from it in not giving rise to mucic acid, and in undergoing fermentation. The conversion of the insipid sugar into grape sugar, by the effect of acids, indicates its place in the series of sugars, and it makes it probable that starch, in its passage through the system, undergoes this change also,

and thus the whole series at present known will be, starch, dextrin, insipid sugar, sweet sugar, vegetable acid, carbonic acid.

After this account of diabetes insipidus, you will never consider cases of excess of urine as cases of this disease, unless you have reason for thinking that tasteless sugar is present. Cases of excess of urine without sugar are well named cases of diuresis. They for the most part are produced by excess of liquid being taken—that is, by excessive thirst. M. Becquerel has given us the term polydipsia. The urine is of very low specific gravity, containing neither insipid nor sweet sugar, not unfrequently containing a small quantity of albumen. The excessive thirst may be caused by excessive dryness of the back of the pharynx, and such cases generally end in extreme emaciation and phthisis. The quantity and specific gravity of the urine immediately determine whether the case is one of diuresis. The symptom of excessive thirst with urine of low specific gravity constitutes the case one of polydipsia.

Torulæ are by no means diagnostic of saccharine urine; but though they form very soon and very plentifully in diabetic urine, yet they may constantly be found in urine which contains no trace of sugar; and though they may lead you to look for sugar, they must never lead you to assert that sugar is certainly present in the urine in which they occur.

A few words regarding the treatment of cases of diabetes. Why in diabetes does the sugar in the blood not pass into the state of vegetable acid, and so on to carbonic acid and water? Why in health does the sugar in the blood rapidly undergo these changes? Our knowledge of the chemistry of the animal system is not sufficient to give as yet any complete answer to these questions. That the alkali in the blood is one of the agents in effecting this change is most probable. The relation of diabetes to acidity points also to the want of

alkali. The probable benefit of ammonia and alkaline phosphate of soda lead to the same conjecture. The tendency of the disease, being to emaciation and phthisis, gives us at present the best indication for rational treatment. The following case will bring this before you:—

G. G——, (Case-book, vol. viii. p. 145,) aged twenty-four; single; labourer, from Tunbridge Wells; admitted into St. George's Hospital, June 13, 1848, York ward; of a full florid healthy appearance; has been ill eight months, and has known three or four others in his neighbourhood affected like himself with "sweet water," which in his own case he knows by tasting it. "There is one now alive near his home with this complaint." As far as he knows, he was quite well eight months ago, but when younger was subject to boils. Increasing debility has obliged him to give up his work. His general health he says is good, except that for the last four years he has had in the spring a sharp pain on exertion in the loins, for which he has been bled; of late the quantity of urine has been seven quarts in the twenty-four hours; thirst and appetite are great; has lost (he thinks) about two stone the last few months; tongue coated and clammy; bowels irregular; generally has a bad, often a sweet, taste in the mouth; also great sourness of stomach and flatulence; no cough. Castor oil, three drachms, immediately; cod-liver oil, half an ounce, liquor ammoniæ, five minims, three times a day; roast slice, mutton chop, beef tea, two eggs; four ounces of bread only.

June 14th.—Thinks he has less thirst; slept well; only rose once in the night; urine less in quantity; tongue cleaner; bowels open; pulse soft. Continue the medicine every six hours.

15th.—Slept well; tongue cleaner; medicine agrees with him well; weighs nine stone. Urine passed in twenty-four hours, about five pints; specific gravity = 1040.

16th.—Less thirst; urine passed in about the same quantity; tongue less furred; less colour in the face; wants more food. Extra meat diet; no vegetables; mutton chops, two eggs, beef-tea.

17th.—Urine, three pints and a half a day; weight, nine stone.

18th—Urine, four pints and a half; specific gravity, 1030; much the same in other respects. To drink less water, and to take the medicine six times a day.

20th.—Urine, four pints in twenty-four hours. To take the medicine eight times a day.

21st.—Rather more thirst; urine three pints and a half in twenty-four hours; tongue cleaner; no cough. Continue the medicine.

22nd.—Urine, three pints in twenty-four hours; very pale; no appearance of oil in it; cloudy from vibriones; specific gravity, 1032.

23rd.—Weighed eight stone, eight pounds. To take six ounces of oil in the day, one ounce every four hours.

24th.—No sickness; urine, three pints. To have four eggs daily.

25th.—Urine, four pints.

26th.—Urine, three pints; specific gravity, 1030. Continue the medicine every three hours; eight ounces of oil daily.

27th.—Urine, three pints.

28th.—Urine, three pints.

29th.—Urine, three pints.

30th.—Urine, three pints; weight, eight stone, eight pounds. Has been taking six ounces of oil, and not eight as ordered.

July 1st.—Urine, two pints and a half; specific gravity, 1032.

2nd.—Urine, two pints and a half. Cod-liver oil, seven

ounces, liquor of ammonia, one drachm, to be taken in milk in the course of the day.

3rd.—Urine, two pints and a half; specific gravity, 1033; pale in colour.

4th.—Ditto, ditto; cod-liver oil, eight ounces.

5th.—Urine the same, with epithelium and oxalate of lime.

6th.—Urine, three pints; high coloured.

8th.—Urine, two pints and a half; weighed eight stone, twelve pounds.

9th.—Ditto.

10th.—Urine, three pints.

11th.—Urine, two pints and a half. Specific gravity 1030.

14th.—Same quantity of urine. Weighed nine stone one pound.

20th.—Went out at his own request; urine still pale, containing sugar. The quantity of water diminished one half, and the specific gravity one-fourth. The appetite and thirst natural.

The very decided difference in the duration of the disease in thin and fat people tends also to the conclusion that a supply of oleaginous substances at least retards the progress of the complaint.

It is highly probable that, by the delicate tests we now possess, sugar will be found to occur in the urine much oftener than it hitherto has been. I know of one case of a gentleman who considered himself as well. By the merest accident he had his urine examined on account of some small masses, which proved to be fatty epithelium of the bladder, which had attracted his attention. Sugar was found to be present. He was a stout man, and used to be subject to excessive perspirations. The urine, whenever it has since been examined, has contained sugar. He remains in good health.

In another case I have known the sugar detectible in the urine for some days, and then altogether disappear, and then return again, but I have not been able carefully to watch the variations in this very interesting case. The sugar appeared to alternate with excessive acidity, and consequent deposit of uric acid. But in other cases of excessive acidity of the urine, with constant deposit of uric acid, I have been unable to find any sugar in the urine made after or before food. The disease which is characterized by excessive acidity of the urine appears to me to be quite as difficult to cure as the disease characterized by sugar in the urine. It may be palliated by alkalies, and for the time that the medicine is taken the uric acid may be prevented from appearing as a sediment, but in one case, two days after the omission of the salines or alkalies, the uric-acid crystals returned, and that notwithstanding the diet was carefully regulated. Little exercise could be taken. When the alkalies were resumed the uric acid crystals again disappeared.

LECTURE XII.

ON THE RELATION OF THE URINE TO THE FOOD AND SYSTEM; GENERAL METHOD FOR ITS EXAMINATION.

We have this morning to consider the relations of the urine to the food and system generally. We have seen that the food contains four classes of substances, and the urine contains the same four classes; we find in it water, nitrogenized substances, non-nitrogenized substances, and salts, that give ashes; but in forms and proportions so different, that they are no longer fitted to be the food of animals, but most fit for the support of vegetable life. The relation of the urine to the general system has not long been clearly seen. The kidneys were supposed to be organs intended for preparing the urine. They secreted it from the blood; for what purpose was not perceived. Every unhealthy state of the urine was attributed to an unhealthy state of the kidney. For example: sugar, and even urate of ammonia in the urine were considered to be caused by some disease of the kidney, and though, on examination, no disease of the kidney was found, still it was thought that the unnatural state of the urine depended on it alone. No clear ideas were entertained of the relation of the urine to the solids, fluids, and gases of the body.

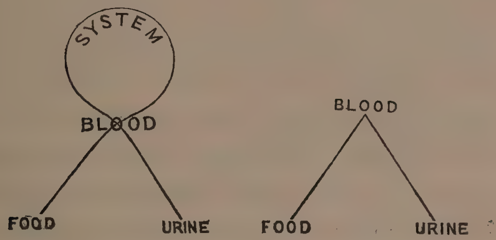
Before any one else without doubt, Dr. Prout, previous to

the third edition of his work in 1840, had formed for himself decided ideas on these questions; but from the terms secondary digestion, vitalization, conversion, reduction, and diathesis, others could not obtain clearness, but only some confused comprehension which no study of Dr. Prout's work could render perfectly distinct. The ideas of Berzelius were but little known here, and it was not until Professor Liebig's work on Animal Chemistry appeared, when, with deep chemical knowledge and great clearness of ideas, he set forth the relations of the urine to the changes in the solids and fluids effected by use, and the action of the inspired oxygen, that the meaning of the language of Dr. Prout became evident. It became self-evident that the kidneys were not to form urine, but to separate from the blood substances that were useless or hurtful to the system. Any organ that is used must be repaired, and the substance that has been used must be removed. Take the muscles, for example; the muscles consist of water, salts, non-nitrogenous fat, and a highly compound arrangement of carbon, hydrogen, nitrogen, oxygen, sulphur, and phosphorus. Carbonic acid, ammonia, water, sulphates, and phosphates are the last products of muscular action, and of the action of oxygen on the muscle. The intervening products probably are innumerable, as kreatin, kreatinine, uric acid, urea, choleic acid. Some of the products are thrown out of the body by the lungs, others by the kidneys. If the removal of some of these products by the lungs is stopped, the circulation through the lungs ceases in two minutes; the heart and brain are stopped, and from the mechanical stoppage in the lungs death ensues. If the removal of these products by the kidneys is stopped, in two days the patient is poisoned; the nerve and muscle are affected by the poison, and chemical death ensues.

If beef-steaks (the muscles of an ox) are given to one who

has taken strong exercise, and is in perfect health, they are dissolved, and pass into the blood, and their chief use is to repair the muscles and nerves, not to form uric acid and urea, and the constituents of the urine. The waste of the muscles and other organs passes off in the urine, whilst the food nourishes the wasting organs. Such, I conceive, to be the clearest ideas I can give you of the relation of the urine to the system and to the food, and theoretically I consider this is the true, healthy relation, and perhaps in a state of full bodily labour, when enough food, and no more food than enough, is taken, this may be the only relation; but provision has been made for too little labour and for too much food. If too much food is constantly taken, and too little exercise, plethora and hæmorrhage must take place, if some escape for the excess of food be not provided. You have seen that the phosphates, sulphates, and urates are generally increased in the urine after food has been taken. If more food is taken than is required for the wants of the system, the excess is thrown out by the same organs that remove the waste of the muscles and other structures. If even excess of water alone is taken, the excess is thrown out partly, at least, by endosmotic laws not yet clearly applied. How the quantity of substances to be thrown out is determined, I do not yet distinctly see. As far as I can understand, it only adds to the difficulty to say that the unvitalized portion of the food or water is thrown out, whilst that portion which is vitalized remains in the blood. When and why uric acid is formed directly from the food seem to me questions more likely to be solved by keeping them distinct from questions of vitalization. Long ago Dr. Prout most fully recognised the fact, that the food not only nourishes the body, but when excess of it is taken passes off in part in the urine. That this double relation of the urine exists, I have also proved to myself in opposition to

the theories I had formed. The facts, then, are these—food makes blood; blood makes muscle; this when used returns in a different form into the blood again, and passes out by the breath and urine. This is the first most healthy systemic or larger circle. The second less healthy relation is caused by excess of food or diminished wants of the system. The circle is smaller. The excess passes into the blood from the stomach, and passes out by the breath and urine.



I consider that we do not know why or how the excess of food is thrown off by the kidneys, but it is not the less certain to me that the food as well as the changes in the body affect the urine.

The relation of the urine to the respiration—that is, to the inhalation of oxygen and the expiration of carbonic acid, must be also considered in order to attain to any clearness of conception as to the nature of the excretion. If the discovery of the circulation of the blood is still considered as exercising the greatest influence on physiology and pathology—if the establishment of this principle is thought to have added more to our knowledge than any other fact previously known—if this be considered as one grand vital action of the human body, then I am sure that the grand chemical principle, the action of oxygen in the body, will before any great length of time be regarded as of equal im-

portance. I will not attempt to compare these actions—indeed, they admit of no comparison; they are related and dependent on each other; they probably cannot even exist separately. The muscle would not contract without the chemical action of oxygen; the oxygen would not reach the muscle without the contractile action of other muscles assisting in circulating it in the blood. There is no spot to which the oxygen does not reach. In the capillaries, in the minute texture of the various organs, the oxygen exercises its power of combining with hydrogen and carbon, giving warmth, and forming compounds, which pass out of the body chiefly by the lungs, the skin, and the urine. In the capillaries of the kidneys, as in other capillaries, oxidation is rapid, and the products of oxidation appear in the urine and other excretions. The colour of the urine, the arrangement of the elements of the effete compounds, perhaps even the acidity, depend on the action of oxygen, though it is possible, as regards the acidity, that in the minute texture of the kidney an action similar to that in the stomach, which causes the liberation of acid there, may take place.

As illustrating the action of oxygen, let me say a few words on the colour of the urine. Yellow, brown, and pink—these are the colours which, in every shade and mixture, are to be met with in the urine. If I asked you where else in nature you could find these three colours in every variety, you would probably answer, in the changing leaves of autumn. The pale-yellow of the poplar, the bright-yellow of the ash, the brown of the oak, the pink of the climatis, the most changing shades of the leaves, could be matched in the urine of health or of disease.

There is a more wonderful comparison than that of colour. The chlorophyle C_{18}, H_8, $N O_8$, or indigo-like colouring-matter of the leaves, exists in three slightly different modi-

fications; when the tree ceases to form them, then new changes take place: the yellow, the brown, or the pink colour, is formed from the green substance. Berzelius, in his investigation of the colour of the bile, has shown that the colouring-matters of the leaves and bile are identical, and subject to the same modifications. Thus the relation of the various colours of the urine to the colour of the bile becomes probable; and the change in the colour of the leaves and the difference between the colouring matter of the urine and that of the bile may both be traced to the action of the oxygen of the air; and thus even the colour of the urine may become an evidence of the oxidation which is going on in the body.

The proof of the relation of the colouring-matter of the bile to that of the urine will be made more probable by my showing you, that sometimes pink and red colouring-matters are formed in the liver. Here, for instance, is a red pigment, collected from a diseased liver; and on one occasion I removed a brick-red gall-stone from the orifice of the common bile-duct. The following notes may be interesting to you:—

Dec 5th, 1842. *Examination of a peculiar brownish-red Concretion, taken from the Extremity of the Ductus Communis.*—When heated on platinum foil an inflammable gas was given off, which burnt with a bright flame, leaving a dark, spongy coal, which, when further heated, burnt of itself, without flame, leaving a perfectly white ash, which was soluble in dilute hydrochloric acid with effervescence, and gave a small precipitate with ammonia, and after evaporation and resolution in water, it gave a considerable precipitate with oxalate of ammonia. The powdered calculus did not effervesce in dilute acid. Boiled with water, the water became slightly greenish. After filtration hydrochloric acid was added, and a slight cloudiness took place. Boiled with

ether and alcohol, scarcely any fat or cholesterine was extracted from it, and these agents became but little coloured. Boiled with dilute liquor ammoniæ, it was dissolved, the solution having a yellowish-green colour. The solution was precipitable by acetic acid. It was far more easily soluble in strong liquor potassæ; the solution was dark greenish-brown, and gave a greenish flocculent precipitate, with acetic acid. When treated with nitric acid the calculus dissolved. The solution was of a deep-red colour, and gave a pale precipitate with water. The calculus was in the museum of St. George's Hospital, but I cannot find it now.

Again: the action of caustic potash on pink-coloured urates evolves the green colour like bile. The colour is totally different from the purple, when liquor potassæ is added to murexid. I believe that purpuric acid has nothing whatever to do with the colouring matter of the urine, but that this latter is closely related to the pink colouring-matter of the bile; and instead of saying that urate of ammonia is mixed with purpurates, or coloured by purpuric or erythric acid, it will be truer to say that it is coloured by pink colouring-matter.

There is a pink colour of the urine, which arises from the conjoint action of rhubarb and alkalies, and which is totally different from the colouring-matter of which I have been speaking. Infusions of rhubarb, and still more markedly of senna, if treated with alkalies, become pink, and the pink colour disappears when the infusion is made acid again. If these substances are taken into the stomach, they pass off in the urine; and if alkalies be given at the same time, I have known, not unfrequently, the colour mistaken for blood, and alarm is sometimes occasioned thereby. Any acid added to the urine will save you from this mistake.

When on the sulphates of the urine, it was shown that sulphur, when taken, increased the amount of sulphates in

the urine. This is another example of the action of oxygen in the system. The sulphur being oxidized, sulphuric acid is formed, and this combines with soda or potash, and passes off in the urine.

Another example of the action of oxygen in the system is seen in the effects of saline draughts on the urine. The ordinary action of such medicines is, to lessen the acidity of the urine. How is this effected? We have taken some vegetable salt of potash: it passes off in part by the kidneys, in the form of carbonate of potash. What has taken place in its passage? Some say the salt is changed in the stomach; that it is there that oxidation occurs, and that carbonate of potash is formed. But if the salt is injected into the veins, (as in some experiments by Dr. H. Hoffman,) the same result ensues, and thence it is evident that the oxidation may occur in the blood, and not in the stomach. If tartrate or citrate of potash are burnt, carbonate of potash and water result, and the same happens when tartrate or citrate of potash are taken into the system.

Very lately, Professor H. Rose, of Berlin, (*Philosophical Magazine*, July, 1849,) has made some most interesting experiments on the inorganic constituents of organic bodies, chiefly as regards their degree of oxidation. He divides the degrees of oxidation into fully oxidized, partially oxidized, and unoxidized. He compares the food, the blood, the flesh, and the urine. The food, if it consists of wheat and other grain, consists of organic substances, the inorganic constituents of which exist partly in an oxidized, partly in an unoxidized state. Vegetable food, then, is partially oxidized. The blood is a partially oxidized body. The flesh is a partially oxidized body, but the quantity of unoxidized matter in the blood is larger than in the flesh, and the quantity of fully oxidized matter is smaller in the blood than in the flesh. The urine is a perfect and fully oxidized sub-

stance. The inorganic constituents of the urine are as highly oxidized as it is possible for them to be.

These experiments furnish a new and beautiful proof of the oxidizing action which is unceasingly taking place in the various parts of the body.

Independently of these examples or proofs of the action of oxygen in the system, I do not think we could see oxygen going in with each inspiration, and carbonic acid coming out with each expiration, without believing that oxygen is continually working great changes in the body. I believe it will be proved, that when there is much change going on in the muscles or nerves, the effect of such changes will be found in the increase of certain combinations of oxygen in the urine. Thus increased action of the nervous system will show itself by the increased amount of the compounds of oxygen and phosphorus; phosphoric acid, combined with alkalies, will be found in excess. And when there is increased muscular action, the compounds of sulphur and oxygen will abound; sulphuric acid will appear in the urine in greater quantity than usual. When there is but little action of oxygen on these tissues, there will be a smaller quantity than natural of these substances in the urine. Such, then, is the best general view I can give you of the relations of the urine to the organs of the body, to the food, and to the respiration.

The urine in disease is not only affected by all these causes of variation, but many diseases—perhaps all diseases —have a peculiar effect of addition or subtraction on the urine. In many diseases, as in diabetes and albuminuria, this effect can be discovered, and we can thereby recognise the disease. Not only diseases of the kidney, but diseases of other organs, or of the whole system, delirium tremens, or inflammation of the brain, fevers, or inflammations, may, by a careful examination of the urine, be recognised. In doubt-

ful cases the value of such information may be very great, and as knowledge extends, the examination of the urine will be more generally made, not only in medical but in surgical cases also. I have known the removal of a doubtful cancer of the lip cause death, which might have been delayed, for a time avoided, by examination of the urine. After death it was remembered that the patient had been in the house previously for diabetes, and was thought to be cured, but I examined the water taken from the bladder after death, and found sugar in it.

I have known the simple removal of a pile cause death, where the patient had albuminous urine, and probably, before long, no operation will be undertaken until it is known whether the urine is healthy or not; and also in the prognosis after an accident, the state of the urine is worthy of attention. The number of deaths after operation, at St. George's Hospital, during the last five years—1844 to 1848—was seventy-two cases, independent of lithotomy; and of these, thirteen cases, or near eighteen per cent., had disease of the kidneys. Moreover, the deaths after slight accidents, during those five years, may, in many cases, be traced to disease of the kidneys.

In conclusion, I will rapidly run over the general mode of proceeding in an examination of the urine.

The urine cannot be well seen, unless in a transparent vessel. A six-ounce phial, filled with the urine and the sediment, if there be any, will be sufficient for every purpose. If possible, the urine, as soon as it is passed, should be put into the bottle.

The first test to be used is litmus paper. The question you ask is—What is the state as regards acidity—not as to the quality only, but as to the quantity? Is it too much, or too little acid? Litmus-paper cannot fully answer this

question. It can tell whether the urine is ammoniacal, or alkaline from fixed alkali, or contains little or much acid, but it cannot tell whether the acidity is more than it should be. Simple inspection of the urine is able to solve this question, and that better than any other mode whatever. There cannot be an excess of free acid in the urine without the uric acid being set free, though this often requires many hours to crystallize out. If, then, you wish to know if the urine is too acid, you must leave the phial at rest for twenty-four, and sometimes ninety-six hours; and if there be too much acid, red crystals of uric acid will be very distinctly seen adhering to its sides, or deposited. The microscope may tell you quicker, but it will not tell you more surely, than the naked eye. Whatever the degree of reddening of the litmus, or the amount of urate-of-ammonia sediment, you cannot with truth speak positively of an excess of acid being present, unless you see uric-acid crystals; and it is only when free acid is present in the urine, that alkaline remedies are absolutely necessary.

Litmus-paper, then, tells if the urine is acid or alkaline. Uric-acid crystals tell if it is too acid; their absence indicates that alkaline remedies are not absolutely necessary.

If the urine is alkaline, heat applied to the litmus-paper will generally tell whether it be from fixed alkali or ammonia. If the former, mineral acids and tonics have the best possible effect; if the latter, inflammation of the mucous membrane is the most probable cause of the change of the urea into carbonate of ammonia.

The microscopic examination of the sediment serves to confirm all these deductions. In the last case, pus-globules and prisms of phosphate of ammonia and magnesia are seen; in the former case, granules of phosphate of lime, more rarely crystalline phosphate of lime, and frequently oxalate-of-lime octahedra.

Here, then, a short examination of the urine tells whether there is local inflammatory disease of the mucous membrane of the urinary organs, or whether the general disorder of the system is to be remedied by giving alkalies or acids.

After examining the acidity of the urine, if clear, the specific gravity is next to be taken, either by a urinometer, or by a bottle and balance. If the urine is of low specific gravity, the probability of albumen existing in the urine should be tested. If the urine is of high specific gravity, the existence of sugar should be suspected, and the more so the paler the urine is.

Having determined the specific gravity, a drop of the urine, with the sediment, should then be examined by the microscope. Pus, mucus, blood, fibrinous casts, urate of ammonia, uric acid, oxalate of lime, phosphate of ammonia and magnesia, may or may not be seen to be present. The drop of urine should then be left to dry on the glass for twelve or more hours, and then again examined, and sugar or urea may be then seen ; and by the ease or difficulty with which the glass can be cleaned, albumen may be proved to be absent or present. The bottle, also, containing the urine should, after standing twelve hours, be again examined. Uric acid, blood, and pus, may then sometimes be more clearly seen by the naked eye. If, from this examination, pus is suspected to be present, the action of liquor potassæ on the sediment should produce ropiness. If fibrinous casts are seen, or adhesive matter forms on the slip of glass, the urine should be filtered and examined for albumen. If sugar is suspected, the sulphate-of-copper test, and the test by boiling liquor potassæ, should be also tried.

Let me again repeat another example of the knowledge which may be derived from examination of the urine.

Let me ask this question. Is there blood or is there no blood in the urine? If there is, does it proceed from the

most frequent disease of the kidney—Bright's disease, or from calculus? The first question, simple inspection of the urine after it has stood in the phial for twelve hours, will generally decide. Usually the blood-globules are insoluble in the saline urine; they subside in twelve hours; and though the eye cannot judge with certainty whilst the blood-globules are suspended, it can judge most accurately when they have fallen, and form a layer at the bottom of the glass. For this no microscope is necessary, though it will more quickly decide this question; and without the microscope, the second question, as to the cause of the blood, cannot be solved except by the general symptoms. If fibrinous moulds of the ducts are found by the microscope with blood, there can be no doubt that the blood is caused by congestion of the cortical structure of the kidney; and if this constantly exists, Bright's disease is present, and the low specific gravity and excess of albumen in the urine will generally help to confirm this deduction; the history of the case always being well considered. If with the blood uric-acid or oxalate-of-lime crystals are found, and the specific gravity is high, and fibrinous moulds are wanting, then most probably a calculus is present in one kidney. Perfect rest will help to confirm or contradict this diagnosis. These instances I might multiply; but these are sufficient to show you the value of examination of the urine for diagnosis and treatment. By such examination, both in serious diseases and in slight disorders, I believe that as much or even more useful evidence will be obtained regarding complaints of the stomach, the kidneys, and the system than has been acquired respecting diseases of the lungs and heart by the stethoscope.

If in these lectures I have given you some idea of the value of the examination of the urine for diagnosis and treatment of stomach and renal diseases—if I have enabled you to obtain clearer ideas of the questions you may ask

and of the answers you may obtain—if I have proved to you that chemical actions are taking place in the body, that the action of oxygen never ceases there—if I may lead any of you to the conclusion, that the vital force is but a collective term, including many forces, as the nervous force, the contractile force, the chemical forces, and the formative forces,—I shall not have occupied your time in vain.

Finally, I have as far as possible endeavoured to base these lectures on experiments. Should any future investigation of others (or of my own) lead me to different results, when they are confirmed, I shall immediately modify or give up my opinions. Whatever I have said that is contrary to the views of other English or foreign chemists has been adopted by me solely because further careful experiments led me to conclusions opposed to theirs.

In chemistry, no other authority than that of experiment can with safety for one moment be trusted; and if any of you, by further and better research, can arrive at truer results, my opinions must not for one moment be permitted then to retard the progress of truth.

THE END.

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